Static and Dynamic Underinvestment: An Experimental Investigation*

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Abstract

In this paper we analyze a stylized version of an environment with public goods, dynamic linkages, and legislative bargaining. Our theoretical framework studies the provision of a durable public good as a modified two-period version of Battaglini et al. (2012). The experimental design allows us to disentangle inefficiencies that would result in a one-shot world (static inefficiencies) from additional inefficiencies that emerge in an environment where decisions in the present affect future periods (dynamic inefficiencies). We solve the first-best solution and compare it to the symmetric stationary subgame-perfect equilibrium of a legislative bargaining game. The experimental results indicate that subjects do react to dynamic linkages and, as such, there is evidence of both static and dynamic inefficiencies. The quantitative predictions of the bargaining model with respect to the share of dynamic inefficiencies are closest to the data when dynamic linkages are high. To the extent that behavior is different from what is predicted by the model, a systematic pattern emerges, namely the use of *strategic cooperation* whereby groups increase the efficiency of current proposals by selectively punishing, in future proposals, individuals who propose highly inefficient allocations.

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1 Introduction

Many important public goods are supplied by the government and thus are determined via a legislative process. Furthermore, most of these public goods are long lived and cannot be appropriately considered in the context of a one-shot decision. Rather, over time, the legislature must repeatedly determine how much resources to allocate to such public goods, and prior investments in the public good have impacts beyond the moment where the investment is made. There have been recent developments in economic theory integrating these factor in models of public good provision with dynamic linkages. Indeed, papers in political economy such as Battaglini and Coate (2008) recognize the importance of dynamic linkages and provide an analysis of these type of situations. Once the setting is a dynamic one, there are multiple channels that generate inefficiencies, or differences between the equilibrium level of public goods and the one a central planner would select. In particular, agents not only wish to free-ride with respect to other agents' contribution in the current period, but also with respect to the contributions of agents in the future.

In this paper we design a stylized version of an environment with public goods, dynamic linkages, and legislative bargaining. The goal is to simplify the environment while maintaining some of the key features present in models such as Battaglini and Coate (2008). More precisely, our theoretical framework studies the provision of a durable public good as a modified version of Battaglini et al. (2012). We develop an experimental design that allows us to disentangle inefficiencies that would result in a one-shot world (static inefficiencies) from extra inefficiencies that emerge in an environment in which decisions in the present affect the future (dynamic inefficiencies). Note that such a question is particularly relevant given the frequent observation that free-ridding is much less severe in public good experiments than the theory suggests. Hence, one may wonder if the more subtle issue of *dynamic* free ridding is something people even take into consideration. The setting put forth to investigate this question is simple. In each of two periods of time, a committee decides on the allocation of a fixed budget over a public good and private consumption for each member. The division is determined by majority rule using the multilateral bargaining procedure of Baron and Ferejohn (1989).[1] The dynamic link is provided by the public good, as a portion $\delta \in (0, 1)$ of the first period investment survives and is available in period 2. In other words, the level of the public good in period 2 equals the portion that survived from period 1 plus period 2 investment. We solve for efficiency and also characterize the bargaining equilibrium, a symmetric stationary subgame perfect equilibrium, which is the most common concept used in applied work.

In the bargaining equilibrium, investment is distorted away from the first best. To see why, consider first the case in which no portion of the public good survives ($\delta = 0$). When the planner decides how to allocate the budget, she considers the benefit that an additional unit of investment has for *all* committee members. With bargaining being settled by majority rule, however, the equilibrium results from computing the investment benefits to a *minimum winning coalition* (MWC). The consequence is underinvestment or static inefficiencies.

[1] One member chosen at random submits an allocation proposal that is then voted on by all committee members. If the proposal does not achieve a simple majority of votes, it is rejected, and the process repeats itself.

When the public good is durable ($\delta \in (0,1)$) there is an obvious incentive for higher investment in period 1, but also new sources of underinvestment. A suboptimal period 1 choice will now affect future choices. In the bargaining equilibrium, the committee will start in period 2 with a lower level of the public good, constraining the set of options for that period with respect to the efficient solution. The planner in a dynamic setting considers the effect that current decisions have on all committee members in the present *and* in the future. But, again, in the bargaining equilibrium, period 1 decisions are only concerned with the present and the future of a subset of members, namely, those in the MWC.

We will use the term *dynamic inefficiencies* to account for any underinvestment that results on top of static inefficiencies, where *static inefficiencies* are the ones that emerge when public goods do not affect payoffs for more than a single period. In the bargaining equilibrium, dynamic inefficiencies can represent a very large portion of total inefficiencies. For example, in our parametrization, if only 80% of period 1 investment survives in period 2, dynamic inefficiencies account for approximately three-quarters of the total inefficiencies. Our theoretical environment provides a very conservative measure of dynamic underinvestment. First, we use a two-period model as it is the simplest environment in which dynamic effects arise, but the inefficiency gap increases with the time horizon. Second, in our experiments committees involve three members, but dynamic inefficiencies increase with committee size. In other words, findings in line with dynamic inefficiency predictions in our setting are suggestive of an even greater role for the practical relevance of dynamic effects in general.

Our experimental design allows us to disentangle the static from the dynamic component. The control treatment sets $\delta = 0$ and will provide us with a measure of static underinvestment. We also study in the laboratory the cases $\delta \in \{0.2, 0.8\}$, so that we can compute dynamic underinvestment in two treatments. Moreover, our treatments allow us to study the comparative statics of δ. As the value of δ increases, dynamic inefficiencies as a share of total inefficiencies also increase in the bargaining equilibrium prediction.

The key results with respect to public good investment in period one can be summarized as follows. In our control treatment ($\delta = 0$), mean and median public good investments are not statistically different from those predicted by theory.[2] When comparing investment against the static benchmark, our evidence suggests that there are two main differences stemming from a dynamic environment. First, there is a sizable presence of period one proposals (up to 60%) that benefit all members equally, with investment levels close to efficiency. In other words, when the benefits of period one investment in the public good increase, the proportion of subjects submitting proposals with MWCs decrease. Second, as δ increase, there is more heterogeneity in period 1 investment.[3] A prominent finding in our dynamic treatments is the presence of strategic cooperation. Most subjects who make proposals with period 1 investment close to efficiency propose a MWC in period 2, i.e. they are not unconditional altruists. On average, those subjects use the period 2 proposal to

[2]Other aspects of behavior are similar to what has been reported in legislative bargaining games without public good (see for instance Fréchette et al. (2003)): a majority of proposals are approved immediately, 70% of proposals involve MWCs, and there is evidence of proposal power but the advantage is well below the theoretical prediction.

[3]Also, in dynamic environments, there is no evidence of proposer power in private allocations.

punish those period 1 proposers who did not select high investment, and reward those who did, by strategically including them in period 2's MWC. Theoretically, this strategy is not subgame perfect. In our data, given the choices of other participants, the payoff difference with respect to those proposing MWCs is not large. Therefore, it is not unreasonable that a large proportion of subjects would select strategic cooperative proposals even after substantial experience with the environment.

Despite the heterogeneity in individual behavior, the bargaining equilibrium prediction on the share of dynamic inefficiencies is quite accurate when dynamic linkages are high. The presence of proposals with investment levels close to the planner's solution reduces the size of inefficiencies when we aggregate the data. However, dynamic inefficiencies as a share of total inefficiencies are at 75%, quite close to the theoretical prediction of 72%. In other words, when dynamic linkages are high, inefficiencies introduced by the dynamic structure represent a sizable portion of total inefficiencies, as predicted by the bargaining equilibrium. When dynamic linkages are relatively low, dynamic inefficiencies become less prominent.

Our work is related to previous work on public good provision in static and dynamic settings. With respect to the former, our baseline treatment provides a setup in which the main theoretical prediction and the efficient solution involve interior investment levels. These features contrast with the usual framework used to study public goods, the linear public good game (or voluntary contribution mechanism – VCM). In that model the dominant strategy and the efficient outcome are at the boundary of the action space, with an equilibrium prediction of no investment and an efficient outcome involving full provision. In such a setup, experiments show that investment in the public good remains positive even when participants have experience (see Ledyard (1995) and Vesterlund (2013)). Recent studies of public goods games modify the original VCM to have an interior solution in dominant strategies (see for instance Menietti et al. (2014)). Our work adds to this literature by also providing a static framework with interior solutions in which to study public good provision. Despite the equilibrium of our game not being in dominant strategies, contributions to the public good in the static treatment are much closer to equilibrium levels than in typical VCM experiments. Similarly studies such as Menietti et al. (2014) report results close to equilibrium. The fact that both ours and other studies find congruent results in this regard highlight the importance of the specific details of the game in earlier results.

Our paper also relates to the literature studying public good provision in committees. Two recent papers investigate public good provision in static environments. Fréchette et al. (2012) implement in the laboratory Volden and Wiseman (2006) model of static public good provision with multilateral bargaining. The authors find that public good provision is closely related to the relative weight that subjects put on the private versus public goods, consistent with the predictions of Markov perfect equilibrium. Christiansen et al. (2014) conduct an experimental study of Jackson and Moselle (2002) model, in which players bargain over a single policy dimension and vary whether or not proposer has an access to a budget (pork) that she can privately allocate among committee members. The authors find that the introduction of private goods increases total welfare and shifts the location of the public policy issue from the median towards the one preferred by the most extreme member, who cares the most about the public policy issue.

There are several papers that study public good provision in dynamic settings. Contributions in this area include Herr et al. (1997), Noussair and Matheny (2000), Lei and Noussair (2002), Battaglini and Palfrey (2012), Battaglini et al. (2012, 2013, 2014a), Saijo et al. (2014) and Vespa (2015).

Of these, the most closely related is Battaglini et al. (2012), which analyzes, both theoretically and experimentally, an infinite horizon model of the accumulation of a durable public good under different voting rules, using a different multilateral bargaining mechanism from the one in the present study. Its main theoretical result, which finds strong support in the experimental data, is that a higher majority requirement for passing proposals leads to more efficient public good investment. Aggregate levels of public investment are close to the predictions of the solution concept most commonly used in the literature (Markov perfect equilibrium) and behavior reflects non-myopic decision making. The motivation for and main contribution of the present paper is different: to study a simple two-period environment where it is possible to disentangle the static from dynamic forces behind underinvestment in a transparent way. This simple two-period framework preserves main trade-offs that are present in the dynamic setting with durable public goods and allows for characterization of more complex history-dependent strategies that have a substantial effect on the intertemporal pattern of investment.[4] While the two studies were designed with very different objectives in mind, there are some interesting similarities. Both studies find that inefficiencies are pervasive, with levels of public good investment significantly below the optimal level. The types of proposals observed are also comparable, with most proposals involving side payments to minimum (or nearly minimum) winning coalitions.

The plan of the paper is as follows: Section 2 outlines the model that serves as our benchmark. Sections 3, 4 and 5 give the experimental design and the results, respectively. A brief discussion of our results is reported in Section 6.

2 Theoretical Framework

As mentioned in the introduction, the model is meant to simplify dynamic models of public good provision while retaining the key strategic tensions of such an environment. The game is a two-period model ($t = 1, 2$) of multilateral bargaining with n (odd) committee members indexed by i, each representing one district. There is no discounting between periods. In period t the committee decides on how to allocate a fixed budget B_t between pork to each member denoted by x_{it}, and investment in a durable public good, I_t. Furthermore consumption and investment must be non-negative in both periods, and there is no borrowing or lending. That is, $x_{it} \geq 0$, $I_t \geq 0$, $\sum_{i=1}^{n} x_{it} + I_t \leq B_t$, $t = 1, 2$. Investment accumulates in time and the resulting stock represents the level of the public good, g_t.

[4] As shown in Section 5, considering history-dependent strategies is also useful for explaining the heterogeneity observed in the experimental data.

The utility of member i in period t is given by

$$U_{it}(x_{it}, g_t) = x_{it} + u(g_t),$$

where $u(g_t)$ represents utility from the public good investment. We assume u is twice continuously differentiable and has standard properties: $u'(g_t) > 0$, $u''(g_t) < 0$ for all $g_t > 0$ and $\lim_{g_t \to 0} u'(g_t) = \infty$. The committee starts with zero stock of public good ($g_0 = 0$) and in period 1 the level of the public good is equal to that period's investment ($g_1 = I_1$). A portion $\delta \in [0,1]$ of the first period's investment survives and is still available in period 2, so the depreciation rate is $d = 1 - \delta$. The stock of the public good in period 2 is given by $g_2 = \delta g_1 + I_2 = \delta I_1 + I_2$. If the public good does not fully depreciate ($\delta > 0$) between periods, the problem is dynamic.

In the remainder of this section we characterize and compare public good investments that arise from the bargaining process with the efficient levels implemented by the social planner. We present here the main trade-offs of the model and refer the reader to Appendix A for complete proofs. Our discussion is focused on understanding the sources and the determinants of the inefficiencies in public good provision due to the dynamic nature of bargaining process.

2.1 Efficient Solution

The Planner chooses investment levels and private allocations for each member so as to maximize welfare of the society (aggregate utility of agents) subject to budget constraint and the rule that governs the accumulation of the stock of public good. Since agents' utilities depend linearly on the private allocations, the planner's solution pins down the efficient level of investment in each period $(I_1^{P^*}, I_2^{P^*})$ but is silent regarding how the remaining funds are distributed between agents in private shares.

$$\max_{\left(\{x_{ij}, I_j^P\}_{j=1,2}^{i=1,..,n}\right)} \left[\sum_{i=1}^{n} x_{i1} + n \cdot u\left(g_1\right) + \sum_{i=1}^{n} x_{i2} + n \cdot u\left(g_2\right)\right]$$
$$\text{s.t. } \sum_{i=1}^{n} x_{i1} + I_1^P \leq B_1 \text{ and } \sum_{i=1}^{n} x_{i2} + I_2^P \leq B_2$$
$$I_1^P \geq 0,\ I_2^P \geq 0,\ x_{ij} \geq 0\ \forall i, j$$
$$\text{where } g_1 = I_1^P \text{ and } g_2 = \delta g_1 + I_2^P$$

The Planner's solution depends on whether and which constraints are binding. If no constraints are binding, then there is an interior solution, $(I_1^{P^*}, I_2^{P^*})$, characterized by two first-order conditions that capture a familiar trade-off:

$$\text{period 2:} \qquad n \cdot u'(\delta I_1^{P^*} + I_2^{P^*}) = 1 \tag{1a}$$
$$\text{period 1:} \qquad n \cdot \left[u'(I_1^{P^*}) + \delta u'(\delta I_1^{P^*} + I_2^{P^*})\right] = 1 \tag{1b}$$

The interior level of public good provision equates in each period the social marginal value of an additional unit of investment and its social marginal cost, which equals 1 in both periods. The marginal benefit of an extra unit of public good in period 2 is simply $n \cdot u'(\delta I_1^{P^*} + I_2^{P^*})$, as it benefits equally all n members of the committee. In period 1, however, there is an additional term, which represents the effect of the public investment in period 1 that partially survives until period 2.[5]

If the solution is not interior, then which constraint is binding depends on parameters (B_1, B_2, δ). When available budgets (B_1, B_2) are sufficiently small the planner allocates all available funds to public good provision. If this is not the case, then depending on the depreciation rate, the planner might choose to allocate a portion of period 2's budget to public investment or distribute all available budget in private shares. For sufficiently high rates of public good survival δ it is efficient not to invest at all in the public good in period 2, while for low δ the efficient solution has $I_2^{P^*} > 0$. In any case, our assumptions on u implies a unique planner solution $(I_1^{P^*},\ I_2^{P^*})$.

2.2 Bargaining Solution

We model the bargaining process following the classical model of Baron and Ferejohn (1989). In each period there is a (potentially) infinite number of bargaining stages. At the beginning of each stage one committee member is chosen at random to make a proposal $(\{x_{it}\}_{i=1}^{i=n}, g_t)$, which is then voted on by all members of the committee. If a simple majority votes in favor, then the proposal is implemented and the period ends. If it is voted down, then another bargaining stage (within the same period) starts with a randomly selected member who submits a proposal and the process repeats itself. There is no discounting between bargaining stages within the same period. A portion of the public investment in period 1 survives until period 2, which creates the link between periods.[6] We focus on the symmetric stationary subgame-perfect equilibria with strategies that are anonymous between periods (legislative bargaining equilibrium hereafter). Given the strict concavity of u the legislative bargaining equilibrium is unique in investment levels $(I_1^{L^*}, I_2^{L^*})$.

The equilibrium of a two-period game shares two main features of the one-period game equilibrium: (1) there are no delays on the equilibrium path as proposals are passed right away, and, (2) conditional on public investment being an interior solution, the proposer enjoys higher private share than any other member of the committee. As before, we discuss here the main forces that govern public investment in each period and refer the reader to Appendix A for the detailed characterization.

Conditional on the public investment in period 1, the maximization problem of a proposer in period 2 involves choosing the cheapest proposal that will pass. There are three alternative routes the proposer can take. The first route is to invest all available funds in the public good, that is, $I_2^{L^*} = B_2$. This route is optimal when the stock of the public good that survived from period 1 is sufficiently low, and such proposals pass with a unanimous vote. The second route is to distribute

[5] Notice that if there is no depreciation ($\delta = 1$), then one of the constraints must be binding because the first order condition for period one reduces to $u'(I_1^{P^*}) = 0$, which cannot arise.

[6] It is straightforward to see that analysis presented below generalizes to any quota voting rule, where passage of a proposal requires at least q supporting votes and $1 \leq q < n$.

all the available budget in private shares to form a minimum winning coalition. This strategy is optimal when the stock of public good is sufficiently high. Finally, for intermediate levels of public stock, the optimal strategy of the proposer involves investing portion of the budget in the public good, rewarding $\frac{n-1}{2}$ randomly chosen members with private shares and appropriating the remaining funds to herself. This interior level of period 2 public investment is characterized by the first order condition

$$\text{period 2:} \qquad \frac{n+1}{2} \cdot u'(\delta I_1^L + I_2^{L^*}) = 1 \tag{2a}$$

The comparison between efficient and bargaining level of public investment in period 2 when both levels are interior is instructive (equations (1a) and (2a)). Both the social planner and a member selected to propose an allocation in the bargaining game weigh the marginal benefit of the public investment against its marginal costs. While the marginal cost of public investment is the same in both situations and equals to 1, the marginal benefits are different. The social planner takes into account the fact that a unit of public investment benefits all n committee members. On the contrary, due to the specifics of the bargaining protocol (majority voting rule), the proposer internalizes the effect on $\frac{n+1}{2}$ members only, herself and $\frac{n-1}{2}$ coalition partners. Thus, the bargaining solution underprovides the public good relative to the efficient solution in period 2. This underprovision is purely static as it is present irrespectively of the survival rate of the public good δ (which is the only dynamic component of our bargaining game). We, therefore, refer to this portion of underprovision of public good as the *static inefficiency.*

The proposer selected in period 1 anticipates how her decisions will impact choices of the proposer in period 2 through the accumulation of the public good that is carried over between periods given the survival rate δ. When $\delta = 0$ all public investment in period 1 depreciates and the two-period legislative game becomes simply the one-period legislative game repeated twice. We refer to the game with $\delta = 0$ as the static game, since in this case there is no linkage between periods.[7] When $\delta > 0$ additional dynamic forces are at play. The first order condition that characterizes the interior equilibrium investment in period 1, $I_1^{L^*}$ is:

$$\text{period 1:} \qquad \frac{n+1}{2} \cdot \left[u'(I_1^{L^*}) + \frac{dV_2(I_1^L)}{dI_1}|_{I_1^{L^*}} \right] = 1 \tag{2b}$$

where $V_2(I_1^L)$ represents the continuation value of the game at the beginning of period 2 before the proposer has been selected. The left hand side reflects the distortions from the planner's solution due to both static and dynamic free rider effects. The first term $(\frac{n+1}{2} u'(I_1^{L^*}))$ represents the marginal benefit of the public good in period 1 to the proposer's coalition of $\frac{n+1}{2}$ voters (again, not the social marginal benefit). This the same static distortion that arises in period 2 and is present irrespectively of value of δ. The second term captures the *dynamic free riding* effect because it takes into account how $I_1^{L^*}$ will affect the proposer's (and her coalition partners') continuation value in period 2.

[7]Recall that we restricted our attention to the stationary sub-game perfect equilibria in which strategies cannot condition on the identities of the committee members in the previous period.

We distinguish between two separate dynamic effects of public investment I_1^L on the continuation value V_2: the direct one, which we refer to as the *durability* effect and the indirect one, which we call the *crowding-out* effect.

The crowding-out effect arises when neither of the budget or feasibility constraints are binding, that is, $I_1^{L^*} < B_1$ and $I_2^{L^*}(I_1^{L^*}) > 0$. In this case an increase in period 1 investment completely crowds out period 2 investment. The intuition behind this movement is that the period 1 proposer can reduce the side payments to coalition members by increasing $V_2(I_1^L)$ (by freeing up more period 2 budget for private allocations), and at the same time raise her own payoff.

If in equilibrium the feasibility constraint in period 2 binds, that is, $I_2^{L^*}(I_1^{L^*}) = 0$, then investment in period 1 will not substitute for investment in period 2 at the margin. Hence in this case, the entire dynamic free riding effect is due to the direct *durability* effect. The portion of public underprovision due to the durability effect is larger when the survival rate δ is smaller.

To summarize, the bargaining solution in both periods underprovides the public good relative to the efficient planner's solution. One portion of this underprovision is static since it arises irrespectively of the ties between periods (captured by parameter δ in our game). The other portion, present only in period 1, is dynamic and arises either due to the direct durability effect or due to the indirect crowding-out effect. In the next section we discuss how one can separate these parts of the free riding problem and estimate their magnitudes.

2.3 Identification of dynamic inefficiencies

This paper aims to determine if subjects react to the dynamic aspects of public goods provision. If they do, are there reactions to the static and dynamic free-ridding incentives similar to what theory predicts as parameters of the environment change? Following the theoretical framework presented above, we focus on the distortions in public good provision in period 1 and propose one natural way to disentangle two source of inefficiencies (static versus dynamic), both of which contribute to the low level of public good investment relative to the efficient solution.

Let Δ_1^S capture the difference in public good investment in period 1 between the efficient and bargaining solutions when the public good fully depreciates between periods ($\delta = 0$). This is pure static inefficiency, since it arises as a result of the proposer taking into account his influence only on the utility of $\frac{n+1}{2}$ members of the committee in the current period and ignores the rest of the legislators. Thus,

$$\Delta_1^S = I_1^{P^\star}|_{\delta=0} - I_1^{L^\star}|_{\delta=0}.$$

When a portion of the public good investment survives between periods, i.e. $\delta > 0$, the difference between the planner's and bargaining investments in period 1 encompasses both static and dynamic inefficiencies. We denote this amount by Δ_1^T and refer to it as the total inefficiencies

$$\Delta_1^T = I_1^{P^\star}|_{\delta>0} - I_1^{L^\star}|_{\delta>0}.$$

Subtracting the static portion of inefficiencies from the total ones gives us the dynamic inefficiencies that arise only in the dynamic setup

$$\Delta_1^D = \Delta_1^T - \Delta_1^S.$$

Depending on the parameters of the game, this dynamic distortion captures either the crowding-out effect (when $I_2^{L\star} > 0$) or the durability effect (when $I_2^{L\star} = 0$).

It is straightforward to verify that $\Delta_1^T(\delta)$ is increasing in δ. In other words, the smaller the depreciation of the public good between periods, the larger is the total inefficiency. Since static inefficiencies do not change with δ, this means that $\Delta_1^D(\delta)$ is also increasing in δ. In other words, the stronger the link between periods (the smaller the depreciation rate) the bigger the portion of underprovision that come from the dynamic rather than the static nature of the bargaining. Furthermore, this comparative static implies that the durability effect is always larger than the crowding-out effect, since as one increases the value of δ the dynamic inefficiencies increase and we shift from the region in which all dynamic inefficiencies are due to the crowding-out effect to the region in which all dynamic inefficiencies are due to the durability effect.

3 Experimental Design

3.1 Parameterization

Our experimental design naturally requires the use of a specific parametric public investment function. In particular, we focus on the power function $u(g) = 5\sqrt{g}$.[8] To create the simplest possible environment, which captures all the forces described in the previous section, we consider committees of three bargainers ($n = 3$) that meet for two consecutive periods. In each period, the committee needs to decide how to allocate a budget of 200 tokens ($B_1 = B_2 = B = 200$) between public good investment and pork to each member of the committee. We conduct all the experiments using the Baron-Ferejohn bargaining protocol described above and document participants' behavior in the legislative bargaining game. We then compare this behavior to the theoretical planner's solution to measure inefficiencies that arise from bargaining using the identification strategy described above.

We conduct three treatments, which differ only in the value of δ, the survival rate of the public investment. The first treatment has $\delta = 0$, and, thus, we refer to it as the static bargaining game and denote it by SB. The other two treatments are dynamic bargaining games, one with a low survival rate, $\delta = 0.2$ (DB$^{\text{low}}$), and one with high survival rate of $\delta = 0.8$ (DB$^{\text{high}}$).[9] These two positive values of δ were chosen in a way that allows us to distinguish two types of dynamic inefficiencies: crowding-out and durability effects.

Table 1 displays the predicted values of public investment and private allocations in each period, and in each treatment as a percentage of the budget. We also present theoretical values for static,

[8] The choice of this functional form was motivated by the desire to choose simple and 'familiar' to subjects function which is easy to describe in the instructions and present graphically.

[9] Recall that the survival rate of public investment is the inverse of the depreciation rate. That is, $\delta = 0$ indicates full depreciation, $\delta = 0.2$ indicates high depreciation, and $\delta = 0.8$ indicates low depreciation rate.

dynamic and total inefficiencies using the planner's solution.[10]

When $\delta = 0$, all period 1 inefficiencies are solely static (see the third column under the SB heading). In this case, the planner allocates an extra 15.5% of the budget to investment. When δ increases to 0.2 and we move to the DB$^{\text{low}}$ game, dynamic inefficiencies emerge and total inefficiencies add up to 27.3% of the budget. In this case the dynamic inefficiencies are entirely due to crowding-out effect since $I_2^{L^*} > 0$. In terms of magnitude, dynamic inefficiencies account for 11.8% of the budget and 43% of the total inefficiencies. The relative importance of dynamic inefficiencies changes dramatically when we further increase δ to 0.8 and move to the DB$^{\text{high}}$ game. In this game dynamic inefficiencies are entirely due to the durability effect ($I_2^{L^*} = 0$) and they account for almost 40% of the budget as well as 72% of total inefficiencies. Although there are differences in magnitudes, dynamic inefficiencies represent a substantial and non-negligible amount of total inefficiencies in both DB$^{\text{low}}$ and DB$^{\text{high}}$ games.

Table 1: Theoretical Outcomes as % of Budget

	Static Barg SB			Dynamic Barg with $\delta = 0.2$ DB$^{\text{low}}$					Dynamic Barg with $\delta = 0.8$ DB$^{\text{high}}$				
	P^*	L^*	Δ_1^S	P^*	L^*	Δ_1^T	Δ_1^D	$\frac{\Delta_1^D}{\Delta_1^T}$	P^*	L^*	Δ_1^T	Δ_1^D	$\frac{\Delta_1^D}{\Delta_1^T}$
Period 1													
public good I_1	28.0	12.5	15.5	44.0	16.7	27.3	11.8	0.43	100	44.9	55.1	39.6	0.72
total private goods X_1	72.0	87.5		56.0	83.3				0.0	55.1			
proposer x_1^{Pr}		58.4			55.6					36.8			
coalition member x_1^{C}		29.1			27.7					18.3			
other x_1^{NonC}		0.0			0.0					0.0			
Period 2													
public good I_2	28.0	12.5		19.4	9.2				0.0	0.0			
total private goods X_2	72.0	87.5		80.6	90.8				100	100			
proposer x_2^{Pr}		58.4			60.5					66.7			
coalition member x_2^{C}		29.1			30.3					33.3			
other x_2^{NonC}		0.0			0.0					0.0			

Notes: P^* denotes Planner's solution, L^* denotes Legislative Bargaining equilibrium, Δ^S denotes Static Inefficiency, Δ^T denotes Total Inefficiencies and Δ^D denotes Dynamic Inefficiency.

We note that the parameters of the game were chosen in a way that gives separation between theoretically predicted investment levels in period 1 in dynamic games (44.9% versus 16.7% of the budget) and at the same time result in a similar average expected payment for subjects. The latter property allows to controls for subjects' incentives between treatments, while the former property is important for interpreting the results of the experiments. The consequence of these parameter choices, however, is that total welfare (the sum of periods' 1 and 2 welfares) in the legislative bargaining equilibrium is almost identical in two dynamic treatments, DB$^{\text{low}}$ and DB$^{\text{high}}$. Hence,

[10] The analysis in the theory section shows that dynamic inefficiencies are driven by the period 1 investment decision. For this reason we measure inefficiencies in Table 1 and derive hypotheses using period 1 investment. Section 4.5 presents the analysis of inefficiencies using aggregate welfare.

our main focus in this paper will be on period 1 investments.

3.2 Experimental Interface and Procedures

We conducted sessions at CASSEL (UCLA) and CESS (NYU) using Multistage software (see Table 2).[11] In each location, subjects were recruited from the general undergraduate pool and each subject participated at most in one session. Sessions consisted of 12 or 15 participants.[12] We refer the reader to Appendix B for a copy of the instructions that subjects received, screen shots, the detailed script of the practice round and the quiz that was conducted to make sure subjects understand the structure of the game and payoffs.[13]

Table 2: Subjects per treatment

Treatment	UCLA	NYU
Static Barg (SB)	45 (3 sessions)	
Dynamic Barg with $\delta = 0.2$ (DB$^{\text{low}}$)	42 (3 sessions)	
Dynamic Barg with $\delta = 0.8$ (DB$^{\text{high}}$)	30 (2 sessions)	24 (2 sessions)

In each session subjects played 10 repetitions of the two-period game and we refer to each repetition as a match. In each match subjects were randomly assigned to groups of three. We describe here the main features of the interface. To reduce the computational difficulties, subjects saw on the screen a graph that depicts how dollars (tokens) invested in the project are converted into payoffs.[14] At the beginning of each period, all subjects were asked to choose how they would distribute the available budget between private allocations and the public investment (referred to as the project investment in the instructions). The instructions emphasized how investment in period 1 can generate payoffs in period 2 for dynamic treatments.[15] After all subjects in a group submitted their proposal, one of the three proposals was selected at random (with equal probability) and presented to all group members for a vote. If the proposal was accepted by a majority of votes (at least two out of three), then the period was over and the group moved on to the second period of the game, in which again all subjects were asked to submit their proposal and one of the proposals was chosen at random. If, however, the proposal was rejected, then the group remained in the

[11] We find no significant differences in the behavior of subjects at NYU and at UCLA.

[12] The two sessions of DB$^{\text{high}}$ that we conducted at NYU and one session of DB$^{\text{low}}$ involved 12 subjects. All other sessions involved 15 subjects.

[13] Upon their arrival to the lab, subjects were sited in a separated cubicles and handed printed instructions. After all participating subjects entered the lab, the experimenter read the instructions out loud and answered any questions that subjects had. After that, all subjects participated in a practice round, during which the experimenter read the script describing the software interface and showed the slideshow with screenshots. Finally, after the practice round, subjects were asked to answer 16 questions about rules of the game. Subjects had to answer all questions correctly to be able to begin the experiment. This quiz was conducted after the practice round and before the beginning of the paid rounds.

[14] Earlier pilot sessions were conducted with a different (less visual) interface. Those data are available upon request. Although the change to a more visual interface was motivated by our worry that the computational demands were high, there is no clear indication that this affected the results.

[15] The instructions included a table that explains for investment levels from 0 to 200 (in intervals of 10), how investment in period 1 will translate into period 2 payoffs (see Charness et al. (2004) for an example of the importance of payoff tables). Moreover, subjects were explicitly asked to go over this table when answering the quiz. Subjects in all three treatments received such a table. An example of this table is presented in Appendix B.

first period and another bargaining stage started in which all members were asked to submit a new proposal. Throughout the experiment, subjects could follow the full history of the experiment in a box at the bottom of the screen. At the end of the session one match was selected at random for payment, earnings in that match were divided by 10 and the resulting figure plus the participation fee ($10) paid to participants in dollars. Average earnings were approximately $30 and each session took about 2 hours.

3.3 Experimental Hypotheses

We use four main hypotheses to organize the experimental results. Our first hypothesis highlights the fundamental difference between dynamic and static bargaining games with respect to public investment in periods 1 and 2. While, naturally, public investment is expected to be the same in both periods in the static game, this is not the case in the dynamic game, in which period 1 public investment is predicted to be higher than period 2 public investment. We call this hypothesis the *horizon effect hypothesis* and summarize it as follows.

$$\textit{Horizon effect hypothesis:}\quad I_1^{\mathrm{SB}} = I_2^{\mathrm{SB}} \text{ and } I_1^{\mathrm{DB^j}} > I_2^{\mathrm{DB^j}} \text{ for } j \in \{\mathrm{high}, \mathrm{low}\}$$

The second hypothesis compares public good investment in period 1 across treatments and asserts that investment in the public good increases with the survival rate of the public good. We refer to this prediction as the investment hypothesis and note that it captures another essential feature of the dynamic game, namely that the benefit of investing in the first period is higher when the depreciation rate is lower.

$$\textit{Investment hypothesis:}\quad I_1^{\mathrm{SB}} < I_1^{\mathrm{DB^{low}}} < I_1^{\mathrm{DB^{high}}}$$

The third hypothesis compares the magnitudes of under-provision across treatments, thus, the name under-provision hypothesis. In particular, the higher the survival rate δ, the bigger the gap between the bargaining and the planner solution.[16]

$$\textit{Under-provision hypothesis:}\quad 0 < \Delta_1^T\big|_{\mathrm{SB}} < \Delta_1^T\big|_{\mathrm{DB^{low}}} < \Delta_1^T\big|_{\mathrm{DB^{high}}}$$

The fourth hypothesis compares the two sources of dynamic inefficiencies that are present in the dynamic treatments. We refer to this prediction as the dynamic inefficiency hypothesis and expect that the durability effect dominates the crowding-out effect.

$$\textit{Dynamic inefficiency hypothesis:}\quad 0 < \Delta_1^D\big|_{\mathrm{DB^{low}}} < \Delta_1^D\big|_{\mathrm{DB^{high}}}$$

[16]Since in the SB treatment there can be no dynamic inefficiencies, total inefficiencies are equal to static inefficiencies. Hence, in the under-provision hypothesis $\Delta_1^T\big|_{\mathrm{SB}} = \Delta_1^S$.

4 Aggregate Results

In this section we present aggregate results. We start by exploring the four hypotheses outlined in the previous section, all of which use period 1 public investment as a measure of inefficiencies (see Sections 4.1 - 4.4). We proceed by exploring the welfare implications of period 1 decisions in Section 4.5, in which we are concerned with the total surplus generated in both periods. In Section 5 we zoom in on the individual behavior of subjects to account for the variation that aggregate data abstracts away from. Since the focus of this paper is on public provision in dynamic environments, most of the aggregate and individual results will concern public investments and total welfare. We refer the reader to Appendix C in which we discuss other characteristics of the bargaining process such as the frequency of delays, the distribution of private allocations between committee members, and the determinants of voting behavior.

4.1 Horizon Effect Hypothesis

The two-period bargaining game analyzed in Section 2 is a relatively challenging environment. On the one hand, behavior in the two periods is interdependent and, on the other hand, behavior is predicted to be different across periods in all but the static treatment. We, therefore, start by assessing whether subjects internalize the fundamental difference between dynamic and static bargaining environments by comparing period 1 and period 2 public investments. According to the horizon effect hypothesis, period 2 public investment in both dynamic treatments is predicted to be smaller than period 1 public investment. This is true for both the efficient solution and the equilibrium bargaining solution as depicted in Table 1. The intuition for this result comes from the fact that while the utility of the public good is the same in both periods, the initial stock of the public good in period 2 is at least as high as the one in period 1 since $\delta > 0$ and public investment in period 1 is non-negative. On the contrary, in the SB treatment, in which $\delta = 0$, the public investment in both periods is expected to be the same.

Table 3: Median Investment in each period as % of the Budget (last 5 matches)

	Static Barg SB		Dynamic Barg with $\delta = 0.2$ DB^{low}		Dynamic Barg with $\delta = 0.8$ DB^{high}	
	Period 1	Period 2	Period 1	Period 2	Period 1	Period 2
Planner's solution	28.0	28.0	44.0	19.4	100.0	0.0
Legislative Barg eq	12.5	12.5	16.7	9.2	44.9	0.0
Observed (all proposals)	10.0	10.0***	31.8*	5.0	55.0	0.0

Notes: The table reports the significance level of a test using a quantile regression where the null hypothesis is that the median equals the legislative bargaining equilibrium. (See Table 13 in Appendix C for further details.) Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

Table 3 displays median investment levels per period in each treatment in the last 5 matches along with the theoretical predictions for the efficient solution and that of the legislative bargaining equilibrium.[17] As apparent from Table 3, our data supports the horizon effect hypothesis as public investment in period 2 is smaller than the one in period 1 in both dynamic treatments and is not different in the static treatment. This is confirmed by statistical tests.[18] Notice also that the observed median public investment in both periods is significantly smaller than the efficient levels chosen by benevolent planner in all three treatments. The one exception is period 2 for the DB^{high} treatment in which the efficient solution predicts zero investment given that a large fraction of public investment from period 1 survived until period 2. We note that qualitative results do not change when one looks at the average investment levels instead of the median ones or at all 10 matches of the experiment rather than the last 5 matches.[19]

Finding 1: *Aggregate data supports the horizon effect hypothesis and indicates that subjects have a basic understanding of the dynamic tensions in the bargaining environment.*

4.2 Investment Hypothesis

Table 4 presents the observed average and median investment in period 1 in the last 5 matches as well as the efficient and bargaining solutions.[20] Observed investment is presented for two categories of proposals. The first category comprises all proposals submitted by all members of each group in the first stage of period 1. The second category includes proposals that satisfy the minimum winning coalition (MWC) condition, which are defined as proposals in which $\frac{n-1}{2}$ members of the committee receive a private allocation of no more than 10% of the budget and $\frac{n+1}{2}$ members receive strictly more than 10% of the budget in private allocations.[21] The subset of proposals that were randomly chosen to be voted on and received a majority of votes looks very similar to the first

[17] For the analysis from now on we restrict our attention to the stage in which a proposal was accepted. For the stage in which a proposal was accepted we consider all proposals submitted by members of the committee. This means that for each match and each period we include in the analysis three proposals by committee, so that no particular period or match is given a higher weight. Because a large majority of first-stage proposals pass, the analysis is not qualitatively affected.

[18] To test for the difference in medians the unit of observation is each proposal per match. That is, for each subject and each match we keep track of the period 1 and period 2 investment proposals, and construct a variable that tracks the difference between period 1 and period 2 investment. We estimate the conditional median of the difference by performing a quantile regression, with the difference in investment on the left-hand side and a constant as a control. The estimated constant is not statistically different from zero in the static treatment, but significant at the 1% level for dynamic treatments. (See Table 11 in Appendix C.) From now on we will use quantile regressions when we provide a test on the median. When we test for differences in means we use a random-effects panel regression with the variable of interest on the left-hand side and a control for the intended test on the right-hand side. In all our tests we cluster standard errors by session. In the text we will use the term 'statistically significant' when we can reject the null at 5%.

[19] When we use mean values, findings are qualitatively unchanged. (See Table 11 in Appendix C.) In the static treatment, the mean of the difference between period 1 and period 2 investment is relatively small (3.8%), but it is statistically significant at the 5% level. In the bargaining treatments differences in investment across periods are also statistically significant, but much larger: 24.3 and 41.1% in the DB^{low} and DB^{high} cases respectively. Results using all matches are presented in Table 12 of Appendix C.

[20] We discuss the evolution of sessions in Appendix C.

[21] Note that the definition of MWC suggests that different members of committees are treated differently with respect to their private allocations: some are included in the coalition while others are not. Thus, proposals that involve investing the whole budget in the public investment, and, therefore, treat all members equally, do not satisfy the MWC condition. Allowing non-coalition partners to receive small shares is standard in the literature.

category of all proposals and, therefore, omitted for brevity.[22]

Table 4: Investment in Period 1 as % of Budget (last 5 matches)

	Static Barg SB			Dynamic Barg $\delta = 0.2$ DB$^{\text{low}}$			Dynamic Barg $\delta = 0.8$ DB$^{\text{high}}$		
	mean	median	st. err.	mean	median	st. err.	mean	median	st. err.
Planner's solution	28.0			44.0			100.0		
Bargaining solution	12.5			16.7			44.9		
Observed									
(a) all proposals	16.7	10.0	13.4	38.7***	31.8*	31.9	55.2**	55.0	32.7
(b) MWC proposals	11.1	10.0***	7.3	18.3	10.0	20.3	29.3***	25.0***	20.1

Notes: The table reports the significance level of a test using a random effects (quantile) regression where the null hypothesis is that the mean (median) equals the bargaining equilibrium. (See Tables 14 and 15 in Appendix C.) Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%.

To test the investment hypotheses outlined above, we decompose it into three pairwise comparisons and test each separately: $I_1^{\text{SB}} < I_1^{\text{DB}^{\text{low}}}$, $I_1^{\text{SB}} < I_1^{\text{DB}^{\text{high}}}$ and $I_1^{\text{DB}^{\text{low}}} < I_1^{\text{DB}^{\text{high}}}$. Furthermore, we test these three inequalities for two categories of proposals (a) and (b) separately, where we take the proposal of a subject as the unit of observation. For category (a) we take all proposals, while for category (b) we take only those proposals that satisfy the MWC requirement. To test differences in the mean, we run a random effects regression that uses the quantity of interest on the left-hand side and varies the right-hand side depending on the specific test. For instance, to test whether public investment in period 1 is higher in the DB$^{\text{low}}$ than in the SB treatment, the right-hand side includes the constant and a dummy variable that takes value 1 if the proposal is from DB$^{\text{low}}$. We always cluster standard errors by session.

Our analysis largely reveals that public investment monotonically increases with the survival rate δ irrespectively of whether one focuses on all submitted proposals or proposals that satisfy MWC condition. The treatment effect is significant at 5% level in all pairwise comparisons except for two cases. The median investment in SB is not significantly different than in DB$^{\text{low}}$, and median investment in DB$^{\text{high}}$ is significantly higher than DB$^{\text{low}}$ at the 10% level (p-value 0.087). In addition, investment levels are generally higher when we compare all proposals to proposals that involve MWCs.[23]

Finding 2: *Investment in the public good increases with the survival rate of the public good as predicted by the investment hypothesis.*

[22] Consistent with the previous literature, we find that a majority of proposals are passed without delay in all three treatments and in both periods of the game. These results are presented in Appendix C.

[23] Details of the statistical tests are available in Appendix C: Table 16 (for the median) and Table 17 (for the mean). The output in tables 18 and Table 19 shows that the conclusions do not change if we use all 10 matches.

4.3 Under-provision Hypothesis

Table 5 presents predicted and observed distortions in period one public investment as well as its decomposition into the static and dynamic components. We use this information to examine the under-provision hypothesis, according to which the total inefficiencies in period 1 public provision monotonically increase with the survival rate of the public good.

Table 5: Total and Dynamic Inefficiencies (as % of Budget)

	Static Barg SB			Dynamic Barg $\delta = 0.2$ DB$^{\text{low}}$					Dynamic Barg $\delta = 0.8$ DB$^{\text{high}}$				
I_1	Planner	Barg	Δ_1^S	Planner	Barg	Δ_1^T	Δ_1^D	$\frac{\Delta_1^D}{\Delta_1^T}$	Planner	Barg	Δ_1^T	Δ_1^D	$\frac{\Delta_1^D}{\Delta_1^T}$
Theory	28.0	12.5	15.5	44.0	16.7	27.3	11.8	**0.43**	100	44.9	55.1	39.6	**0.72**
Proposals													
(a) All													
mean		16.7	11.3		38.7***	5.3	-6.0	-		55.2**	44.8	33.5	**0.75**
median		10.0	18.0		31.8*	12.2	-5.8	-		55.0	45.0	27.0	**0.60**
(b) MWC													
mean		11.1	16.9		18.3	25.7	8.8	**0.34**		29.3***	70.7	53.8	**0.76**
median		10.0***	18.0		10.0	34.0	16.0	**0.47**		25.0***	75.0	57.0	**0.76**

Planner: Planner's solution, Barg: Legislative Bargaining solution

Δ_1^S : Static Inefficiencies, Δ_1^D : Dynamic Inefficiencies, Δ_1^T : Total Inefficiencies

Observed Δ_1^S, Δ_1^T computed using theoretical values for the planner's problem.

In each category (a) and (b) we use first period investment in the last five matches

For the Bargaining column in each treatment, the table reports the significance level of a test using a random effects (quantile) regression where the null hypothesis is that the mean (median) equals the bargaining equilibrium. (See Tables 14 and 15 in Appendix C.) Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Table 5 shows that the estimated magnitudes of distortions are sensitive to the category of proposals one focuses on. If we constrain proposals to those involving MWCs (category (b)), then we find support for the under-provision hypothesis. In other words, proposals that satisfy the MWC condition exhibit the following pattern: the total amount of under-provision in these proposals relative to the efficient solution increases with the survival rate of the public good and this increase is statistically significant between any two pairs of treatments at the 5% level.[24]

However, the picture is different if one looks at all submitted proposals. For this category, the under-provision hypothesis holds only partially. The total amount of inefficiency is significantly higher in the DB$^{\text{high}}$ treatment than in either the static SB treatment or the dynamic DB$^{\text{low}}$ treatment at the 1% level. However, we do not observe a significant change in the total inefficiencies when moving from $\delta = 0$ to $\delta = 0.2$, which corresponds to SB and DB$^{\text{low}}$ treatments respectively.

Finding 3: *Consistent with the hypothesis, public good under-provision in the DB^{high} treatment is*

[24] We use the same statistical analysis described in Section 3.2. That, is we use a random-effects panel regression to test differences in mean Δ_1^T and quantile regression analysis to test differences in median Δ_1^T. Tables 20 and 21 in Appendix C show the output.

higher than in the SB and DB^{low} treatments. Under-provision is statistically higher in DB^{low} than in the SB treatment for MWC proposals.

4.4 Dynamic Inefficiency Hypothesis

Our last hypothesis investigates the presence and relative magnitude of the dynamic inefficiencies in DB$^{\text{low}}$ and DB$^{\text{high}}$ treatments. To statistically examine the presence of dynamic inefficiencies we run a random effects regression for each DB treatment with period 1 investment on the left-hand side.[25] In the DB$^{\text{high}}$ case, the right-hand side variables are a constant and a dummy that takes value 1 if the proposal corresponds to the DB$^{\text{high}}$ treatment and 0 if it corresponds to SB. We then contrast the estimated coefficient with the theoretical increase in the planner's investment relative to the SB case, which equals 72% (100% − 28%). The estimated coefficient on the dummy using all proposals is 38.5% (19.2% using proposals that satisfy the MWC condition), which is significantly less than 72% at the 1% level (using either all submitted or all MWC proposals). Thus, there is evidence of dynamic inefficiencies in the DB$^{\text{high}}$ treatment. In a similar analysis of the DB$^{\text{low}}$ treatment, the estimated coefficient implies that investment in the DB$^{\text{low}}$ treatment is 22% higher than in the SB if we use all proposals (7.6% if we only use MWC proposals). The theoretical increase in the planner's investment is 16% (44%−28%). Therefore, for the DB$^{\text{low}}$ treatment, we find a statistically significant difference for MWC proposals.[26]

While we expect dynamic inefficiencies to be present in both dynamic treatments, the source of the dynamic inefficiencies in each treatment is different, as discussed in Section 2.2. In the DB$^{\text{high}}$ treatment, the dynamic portion of under-provision is attributed to the durability effect, which arises when feasibility constraint in period 2 binds and becomes more important as the survival rate δ becomes smaller. On the contrary, in the DB$^{\text{low}}$ treatment dynamic under-provision is entirely due to the crowding-out effect, which arises when neither the budget nor feasibility constraints bind, and becomes smaller as δ decreases. For the parameters used in our experiments, the theory predicts that the durability effect should be larger than the crowding-out effect. As evident from estimated coefficients reported in the previous paragraph, our data indicates that the observed durability and crowding-out effects are, respectively, smaller than the corresponding theoretical prediction. Despite the fact that the observed magnitudes of the two effects are lower than the predicted ones, we find that the two effects obey the ranking predicted by theory: indeed, the durability effect is larger in magnitude than the crowding-out effect both when we focus on all submitted proposals as well as only the MWC proposals.[27]

[25] The output of these regressions is presented in Table 17 of Appendix C.

[26] The conclusions are similar if we focus on median investment instead (see Table 16 of Appendix C) or all matches (see tables 18 and 19 of Appendix C).

[27] We run a random effects regression where the dependent variable is period 1 investment and the right-hand side involves a constant and a dummy that takes value 1 if the proposal comes from DB$^{\text{high}}$ and zero if it comes from DB$^{\text{low}}$. The coefficient of the dummy variable is significant at the 1% level if we use all submitted or all MWC proposals. The same finding holds if we focus on median investment. See tables 16 and 17 of Appendix C for details. In addition, we can contrast the coefficient on the dummy variable to the theoretical increase in optimal investment. In DB$^{\text{high}}$, optimal period 1 investment is 100%, while on DB$^{\text{low}}$ it equals 44%. Hence, there would be evidence of no additional dynamic inefficiencies in DB$^{\text{high}}$ if the coefficient on the dummy variable was equal to 56%. This null

Finding 4: *Dynamic inefficiencies due to durability effect are significant and large in magnitude in the* DB^{high} *treatment irrespectively of whether one focuses on all submitted or MWC proposals. In the* DB^{low} *treatment, we observe significant dynamic inefficiencies due to crowding-out effect only for MWC proposals. Moreover, consistent with the theory, we find that the durability effect dominates the crowding-out effect.*

4.5 Welfare

Up until now, the analysis has focused on period 1 investment decisions, which are a key determinant of final welfare of a group as explained in Section 2. Other things equal, a sub-optimal period 1 investment would lead to a welfare loss as measured by total surplus generated by a group in both periods. In other words, a dynamic inefficiency in terms of public investment would correspond to a dynamic inefficiency in terms of welfare. However, it is possible that subjects react in period 2 to sub-optimal period 1 investment in a way that may at least partially offset welfare losses from the period 1 underinvestment. In this section we investigate the possibility that such compensation occurs by defining a welfare measure that incorporates public investments in both period 1 and period 2.

Define the measure of a group's welfare as the additional surplus generated on top of the minimum possible welfare that a group is guaranteed to achieve, absent any public investments in either period. Specifically, this measure, W, is defined as:

$$W = \sum_{i=1}^{3} x_{i1} + 3u\left(g_1\right) + \sum_{i=1}^{3} x_{i2} + 3u\left(g_2\right) - 2B.$$

In other words, W adds up payoffs of all three group members in both periods and subtracts $2B$, which is the lowest possible total payoff that a group can obtain by distributing all available budget in private shares and not investing at all in either period.

This welfare measure can also be used to break down the total efficiency losses into those attributable to static and dynamic inefficiencies, in a similar way to the earlier analysis of period 1 investment decisions. The results of this alternative approach are presented in the first row of Table 6. First, for each treatment separately, the table presents theoretical values for W both in the efficient and in the legislative bargaining solution (denoted W^{P^*} and W^{L^*}, respectively). Second, the table shows the total inefficiency relative to the planner's solution (ΔW^T) and decomposes it into static and dynamic components (ΔW^S and ΔW^D, respectively). The results are very similar to the calculations presented in Table 5, which reported the same analysis, but with respect to period 1 public investments instead of W. In particular, the under-provision hypothesis can be stated in terms of welfare instead of period 1 decisions as $\Delta W^S < \Delta W^T|_{\mathrm{DB^{low}}} < \Delta W^T|_{\mathrm{DB^{high}}}$. Similarly, the dynamic inefficiency hypothesis can be stated using the welfare-based measure as $0 < \Delta W^D|_{\mathrm{DB^{low}}} < \Delta W^D|_{\mathrm{DB^{high}}}$.

hypothesis is rejected at the 1% level for the median and the mean. Again, see tables 16 and 17 of Appendix C for details.

Before discussing our findings, it should be noted that a potential drawback from using W as a measure of inefficiencies instead of period 1 investment is that it is based on many fewer observations, for the following reason. In order to measure W one must include period 1 and period 2 proposals, and hence only period 1 proposals that were randomly selected (and passed) can be used.[28] For period 1 proposals that were either not selected or failed to pass one does not observe the period 2 proposals they would have triggered. Hence, one must drop period 1 proposals that were not selected for a vote, or failed to pass, which represents more than two-thirds of the period 1 investment data.[29]

Table 6: Welfare measure: theory and observed outcomes

	Static Barg SB			Dynamic Barg $\delta = 0.2$ DB$^{\text{low}}$					Dynamic Barg $\delta = 0.8$ DB$^{\text{high}}$				
	W^{P^*}	W^{L^*}	ΔW^S	W^{P^*}	W^{L^*}	ΔW^T	ΔW^D	$\frac{\Delta W^D}{\Delta W^T}$	W^{P^*}	W^{L^*}	ΔW^T	ΔW^D	$\frac{\Delta W^D}{\Delta W^T}$
Theory	112.5	100	12.5	126.6	109.9	16.7	4.2	0.25	201.9	179.4	22.5	9.9	0.44
Proposals													
(a) All													
mean		91.5***	21.0		97.6**	29.0	8.0	**0.28**		169.4*	32.5	11.5	**0.35**
median		94.2*	18.3		107.9	18.7	0.4	**0.02**		180.1	21.8	3.5	**0.16**
(b) MWC													
mean		92.1***	20.4		89.4***	37.2	16.8	**0.45**		167.0***	34.9	14.5	**0.42**
median		94.2***	18.3		103.5	23.1	4.8	**0.21**		169.6	32.3	14.0	**0.43**

W^{P^*}, W^{L^*} Welfare Measure under the Planner's and Legislative Bargaining solution, respectively
$\Delta W^S = W^{P^*}|_{\text{SB}} - W^{L^*}|_{\text{SB}}$ (Static Inefficiencies), $\Delta W^T = W^{P^*}|_{\text{DB}} - W^{L^*}|_{\text{DB}}$ (Total Inefficiencies),
$\Delta W^D = \Delta W^T - \Delta W^S$ (Dynamic Inefficiencies)

Observed $\Delta W^S, \Delta W^T$ computed using theoretical values for the planner's problem.

In each category (a) and (b) we use mean investment in the last five matches

For the Bargaining column in each treatment, the table reports the significance level of a test using a random effects (quantile) regression where the null hypothesis is that the mean (median) equals the bargaining equilibrium. (See Tables 22 and 23 in Appendix C.) Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Table 6 presents efficiency levels observed in each treatment as well as their decomposition in terms of static and dynamic under-provision. We present data for all proposals (rows 2 and 3) as well as only for the proposals that involved minimum winning coalitions in both periods (rows 4 and 5). Reported results are qualitatively in line with the predictions and the findings previously reported. Consider first the under-provision hypothesis, which ranks inefficiencies between treatments from the smallest in the SB treatment to the highest in the DB$^{\text{high}}$ treatment. The data provide qualitative support of this hypothesis, as the highest average and median level of inefficiencies are observed in the DB$^{\text{high}}$ treatment, and the lowest average and median level in the SB treatment, when focusing

[28]If we computed a partial measure using only period 1 proposals, part of the welfare consequences of suboptimal period 1 choices would be missing. More importantly, the missing portion would differ across treatments, as the effect of period 1 choices on period 2 welfare depends on δ.

[29]For each period 1 proposal that passed we can compute W for all period 2 proposals (those that were selected for a vote and passed and those that were not).

on all proposals.[30] When we restrict our attention to proposals that satisfy requirement of MWC in both periods, the ordering is preserved for the median levels of inefficiencies and reversed for the average level of inefficiencies in the two dynamic treatments. We note that the comparison between means in the two dynamic treatments is severely affected by a few outliers.[31] Consider now the dynamic inefficiency hypothesis, which predicts that the magnitude of dynamic inefficiencies in the DB^{high} treatment should be higher than that in the DB^{low} treatment. Except for the means of the proposals that satisfy MWC in both periods, in all other cases the data support this hypothesis.[32]

Finally, observe that period 2 public investment decisions do not compensate for the suboptimal public investments in period 1. For this reason, measuring inefficiencies using W leads to similar qualitative conclusions as measuring inefficiencies using period 1 public investments.

5 Individual Data Analysis

Our previous results considered aggregate level data. These results suggest relatively high degree of heterogeneity in public investment decisions across subjects (see, for example, the standard errors reported in Table 3). In this section we look at the individual level data with the aim of documenting and studying in more depth the main types of strategies used by our subjects. This section is structured as follows. First, we define three types of strategies and show that these types capture the vast majority of the observed public investments. Then we document the popularity of each type of strategy and look at their prevalence as the sessions evolve. Second, for each type of strategy we compare public investments in both periods to the optimal investments conditional on the type of strategy used. Third, we consider payoffs associated with the use of each type of

[30] To perform the statistical tests, we use the same statistical types of regressions as the one described in Section 4.3. We find that mean total inefficiencies in DB^{high} are significantly higher at the 5% level than those in SB treatment using all proposals or those that satisfy MWCs. The same finding holds for the comparison between the DB^{low} and SB treatments, with a p-value of 0.069 for the comparison between means. For other comparisons the differences are not statistically significant at standard levels. See tables 24 and 25 in Appendix C for more details.

[31] Notice first that the reported averages are not in line with the prediction for the MWC proposals, with the highest total inefficiency achieved in the DB^{low} treatment. An observation involves a MWC if the period 1 and the period 2 proposals involve MWCs. This requirement further reduces the dataset in addition to looking only at the period 1 proposals that passed, and eventually a few outliers can substantially affect the average. In the DB^{low} treatment, there are 67 MWC proposals. Six of these proposals involve no investment in either period, leading to a W of zero. If these six proposals are excluded, the mean moves from 37.2 to 28.4, and the ordering of total inefficiencies is again in line with the theoretical predictions. The outliers almost do not affect the median, which moves from 23.1 to 22.5. In terms of statistical tests, the results for the mean depend on the outliers. If the outliers are included, total inefficiencies in DB^{low} are substantially higher than in SB, but not different than in DB^{high} (see Table 25 in Appendix C). If the outliers are excluded, then there is no significant difference between DB^{low} and SB. The evidence suggests that the significance of estimates is affected by sample size as well. Tables 26 and 27 show the estimates using all 10 matches but excluding the outliers where $W = 0$ (that are only present in DB^{low}). Median total inefficiencies in MWC proposals are higher in DB^{high} than in DB^{low} (5% level), and in DB^{high} relative to SB (at the 1% level). A similar finding holds for the mean.

[32] In order to provide a statistical test we proceed as in Section 4.4. To establish if there are dynamic inefficiencies in DB treatments, we run a random effects regression for each DB treatment, where W is on the left-hand side and a treatment dummy (1: corresponding DB treatment, 0: SB) on the right-hand side. When then compare the estimated coefficient to the theoretical increase in the planner's welfare (201.9-112.5=89.4 in DB^{high}, and 126.6-112.5=14.1 in DB^{low}). For DB^{high} we find that either using all proposals or those that satisfy MWC we can reject the null hypothesis (at the 5% level) that the coefficient is equal to 89.4. For the DB^{low} treatment, we can reject the null at the 5% level for MWC proposals and at the 10% for all proposals. See Table 28 in Appendix C for details.

strategy and study whether these payoff differences can account for the differences in the use of the strategies between treatments. Finally, we provide some insights into the rationale for the observed strategies.

5.1 Types of strategies and evolution of their use

A strategy in our two period dynamic game is a proposal in period 1, and a period 2 proposal for each of period 1's possible outcomes. In our data we partially observe period 2 choices, as we learn each subject's period 2 proposal only for the proposal that passed in period 1. In this section, the unit of observation will be the subject's choice in both periods of the match and, even though this is an abuse of terminology, we will refer to it as the subject's strategy. There are three types of strategies that account for the vast majority of observed choices:

- *MM strategies*
  Involve forming MWCs in both periods (hence the name MM), where MWC proposals are defined as above.

- *EE strategies*
  Involve splitting benefits equally between all three members in both periods (hence the name EE), where we define as an equal split any proposal in which the difference in private allocations between any two members is not larger than 5 tokens (2.5% of the budget).[33]

- *EM strategies*
  Involve splitting resources equally in period 1 and forming MWC in period 2 (hence the name EM).

Figure 1 plots the proportion of subjects who submit a proposal of each identified strategy type by match in each treatment. The first observation is that classifying proposals into the three strategy types outlined above accounts for the vast majority of all observed choices: 88% in SB game and 94% in either of the DB games in the last 5 matches (see also the dotted line that represents the total number of observations that fall into one of the defined types).

The popularity of each strategy type varies with the treatment and evolves as subjects gain experience with the environment. However, in all three treatments, the EE strategy loses its popularity to MM and EM strategies as session evolves. Indeed, while EE type strategies are the most common in all treatments at the beginning of the session, there is a clear decline in their use by the end of the session especially in the SB and the DB^{high} treatments.[34] The two other types of strategies gain popularity as subjects gain experience. In the SB game, by the end of the session

[33]Formally, let x_{it} represent the share of the budget corresponding to a private allocation to subject i at period t according to some proposal. We say that proposal at time t splits equally benefits (or that all members are included in the proposal) if $|x_{it} - x_{jt}| \leq 2.5\%$ for all i, j in the committee. The reason that we allow for small deviations from the exact equal splits is that the total budget of 200 is not divisible by three.

[34]The decline in popularity of the EE strategy over time is reminiscent of: 1) In Fréchette et al. (2003) the authors observe that distributions offering an equal division of payoffs decreases in popularity with experience. 2) In VCM experiments, contributions decline with experience, i.e. outcomes become less efficient over time.

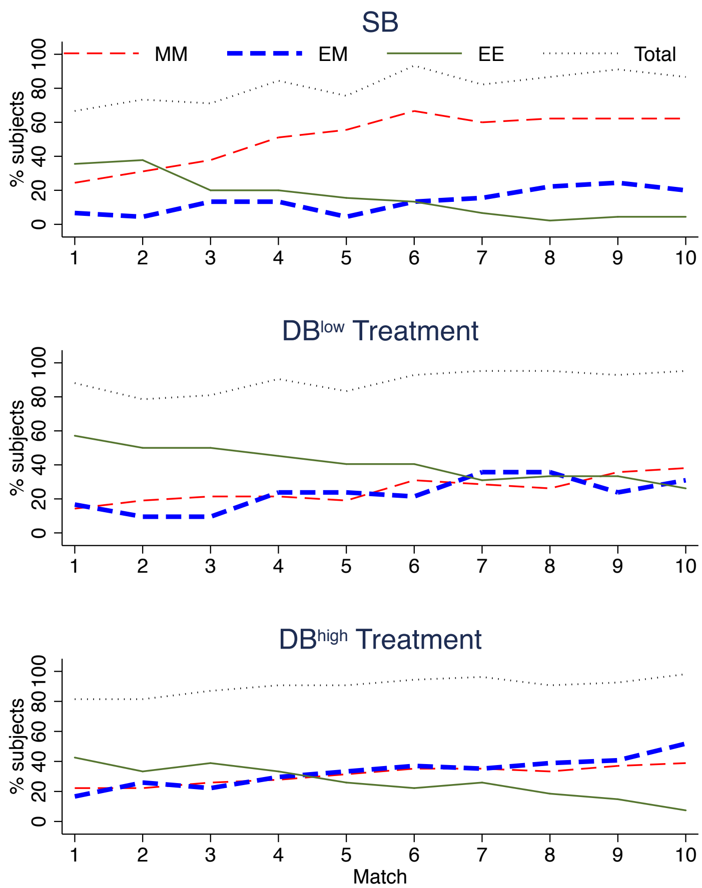

Figure 1: The evolution of strategies used by subjects in each treatment

almost 70% of all strategies are of type MM, while in the DB treatments, about one-third of all proposals are of type MM. Type EM strategies are relatively more popular in the DB treatments, accounting for 30%-40% of all proposals in the last five matches.[35]

5.2 Behavior within each strategy type

Each of the three types of strategies identified above entails very different public investment and allows, in principle, for heterogeneous behavior within the category. For example, for the MM strategy, infinitely many proposals satisfy the MWC condition in both periods, yet of all such proposals the bargaining equilibrium identifies one as optimal behavior given imposed restrictions of symmetry, stationarity and anonymity between periods. Similarly, conditional on using the EE strategy, optimal behavior involves choosing the efficient level of public investment and distributing the remaining funds equally.[36] Finally, the EM strategy combines elements of the bargaining equilibrium and the efficient planner's solution. Thus, conditional on using the EM strategy, optimal behavior involves choosing public provision at the efficient level in period 1 and following the prescription of bargaining equilibrium for public provision in period 2.

Given the prevalence of all three types of strategies in our data, the first natural question is whether, conditional on using a particular type of strategy, subjects choose investment levels that are close to the theoretical ones described above. To address this we calculate the theoretically optimal proposals within each category and compare it to the observed proposals conditional on the type of strategy used. The results are presented in Table 7.

Table 7 reveals several interesting patterns. First, investment levels are very different between proposals of different types. In particular, subjects using EM or EE type of strategies invest a significantly higher share of resources in the public good in period 1 than those that use MM strategies. Moreover, within each category, public investment in period 1 preserves a monotonic relation with respect to the survival rate δ, that is, public provision increases with the survival rate.[37] Finally, in SB and DB^{low} treatments, for each of the three types of strategies, public investments track closely theoretically optimal levels in both periods.[38] On the contrary, in the DB^{high} treatment, we observe under provision of public good in period 1 and over provision of public good in period 2

[35]The analysis of the transitions between strategies reveals that subjects that play MM strategies in the static treatment and MM or EM strategies in the DB^{high} treatment are very likely to stick with this type of strategy. Conditional on changing the type of strategy used, the most popular transitions are from EM to MM type of strategies and from EE to EM type of strategy. The detailed analysis of the transitions between strategies is presented in Appendix D.

[36]Notice that the EE strategy cannot be supported as the sub-game perfect equilibrium in a finite period dynamic bargaining game, as treating committee members equally in the last period is not optimal.

[37]Within each category, the difference is significant at least at the 5% level in all but two cases. For proposals of type EE when comparing between the DB^{low} and the DB^{high} treatments and for proposals of type MM when comparing between SB and the DB^{low} treatments the difference is significant at the 10% level. See Table 39 in Appendix C for details.

[38]Even though Table 7 does report statistical differences between the median and the theoretical predictions in some cases, quantitatively the differences are relatively small.

Table 7: Investment as % of Budget

Treatment	Type MM				Type EM				Type EE			
	theory	mean	median	sd	theory	mean	median	sd	theory	mean	median	sd
Period 1												
SB	12.5	12.8	10.0**	9.1	28.0	27.5	25.0***	11.0	28.0	31.0	25.0***	13.6
DB^{low}	16.7	17.9	10.0***	20.7	44.0	56.8	50.0	34.9	44.0	49.6	40.0	30.6
DB^{high}	44.9	33.2	30.0***	19.6	100	75.5	85.0	28.5	100	70.6	70.0	28.8
Period 2												
SB	12.5	11.9	10.0**	8.6	12.5	12.7	10.0	13.4	28.1	30.5	25.0***	23.6
DB^{low}	9.2	8.7	0.0***	15.3	9.2	6.8	0.0***	13.0	19.3	32.0***	25.0*	29.9
DB^{high}	0.0	13.2***	5.0	18.0	0.0	7.4***	0.0	15.7	0.0	44.4***	29.5***	31.5

The table reports the significance level of a test using a random effects (quantile) regression where the null hypothesis is that the mean (median) equals the theory prediction. (See tables 35, 36, 37 and 38 in Appendix C.) Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

relative to the conditional optimal levels, irrespective of the type of strategy used by the subjects.[39] Notice that in the DB^{high} treatment in all but one case the conditionally optimal public investment is a corner solution (the exception is optimal period 1 public provision for MM strategy). Therefore, any deviations due to mistakes and learning will necessarily be in the direction of underinvesting in period 1 and over-investing in period 2, which is precisely what our data suggests.

5.3 Payoffs for each type of strategy

Table 8 displays information on payoffs by treatment, period and strategy type. For each proposal we compute the payoff for the proposer, for a non-proposer who is in the coalition and for a member who is not in the coalition, whenever the proposal involves a MWC. The table reports the theoretical prediction, the mean and the median using data from the last five matches.

The information on payoffs suggests why the popularity of type EE strategies decreases as the session evolves. Note that period 2 payoffs for proposers are lowest for those submitting type EE strategies, and experiencing low period 2 payoffs can disincentivize using this strategy type. Indeed, Table 30 in Appendix D confirms that a large majority of subjects who are using type EE strategies by match 6 switch to either of the other two characterized strategy types for the last 4 matches of the session.

Inspecting payoffs also provides a rationale for why both, type MM and type EM strategies, persist. We do observe some differences in payoffs across these two strategy types: Payoffs for type MM strategies are higher for those in the coalition, while type EM strategies allow for higher period 2 payoffs given the relatively higher investment in period 1 (most notably in the DB^{high} treatment). However, differences between strategies are small if we compute total payoffs, adding period 1 and

[39]Standard deviations reported in Table 7 indicate that there is some heterogeneity between subjects using the same type of strategy. Figure 6 in the Appendix D provides a closer look at the investment distributions for proposals of type MM and EM. The SB treatment represents the most accurate case, with the mode of the distribution at around the theoretical prediction for both MM and EM strategies. In the DB treatments, there is more heterogeneity for both types of proposals.

Table 8: Payoffs in tokens (last 5 matches-all submitted proposals)

		Type MM			Type EM			Type EE		
		theory	mean	median	theory	mean	median	theory	mean	median
Period 1										
SB	Proposer	141.8	113.6***	112.4***	85.4	84.9	85.4	85.4	84.9	85.4
	Non Proposer	83.2	108.4***	111.5***	85.4	84.9	85.4	85.4	84.9	85.4
	Not in Coalition	25.0	23.9	22.4						
DB^{low}	Proposer	140.1	106.1***	108.7***	84.3	80.1**	83.1	84.3	80.3*	82.4*
	Non Proposer	84.5	104.8***	105.8***	84.3	79.6*	82.4	84.3	80.2*	82.4*
	Not in Coalition	25.0	24.7	22.4						
DB^{high}	Proposer	121.0	106.2***	108.7***	70.7	77.3***	75.2***	70.7	76.6***	75.2***
	Non Proposer	84.2	104.5***	106.5***	70.7	76.9***	75.2***	70.7	76.6***	75.2***
	Not in Coalition	47.4	36.4**	35.4***						
Period 2										
SB	Proposer	141.8	114.1***	112.4***	141.8	109.4***	111.7***	85.4	84.5	85.4
	Non Proposer	83.2	107.6***	109.4***	83.2	108.1***	111.6***	85.4	84.5	85.4
	Not in Coalition	25.0	22.8	22.4	25.0	19.4**	22.4			
DB^{low}	Proposer	145.2	114.0***	114.9***	150.2	116.1***	114.9***	91.3	88.8**	88.5**
	Non Proposer	85.6	112.7***	113.5***	90.6	115.4***	114.9***	91.3	88.6**	88.5**
	Not in Coalition	25.0	22.7*	22.4	30.0	25.1*	22.9***			
DB^{high}	Proposer	175.8	140.0***	137.4***	196.4	148.0***	152.9***	129.8	106.6***	104.0***
	Non Proposer	109.0	138.1***	136.6***	129.8	147.1***	149.9***	129.8	106.2***	103.9***
	Not in Coalition	42.4	45.6*	44.7	63.2	53.5***	58.3**			

Exchange rate: 10 tokens = $1.

The table reports the significance level of a test using a random effects (quantile) regression where the null hypothesis is that the mean (median) equals the theory prediction. Significance indicates a statistical difference from the prediction: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

period 2. Consider the DB^{high} treatment. The payoff of a type MM strategy is 190 tokens (before the identity of the proposer is revealed), which is only slightly below the 193 tokens corresponding to a type EM. Thus, for the two strategy types that corresponds to most of our data in later matches, the difference in payoffs is relatively small.[40] This suggests that the choice between type MM and type EM strategies is not based on a difference in payoffs. In the next section we explore a rationale for selecting type EM strategies.

5.4 A rationale for type EM strategies

To illustrate the rationale, consider an EM strategy that involves public investment at the planner's level in period 1 and dividing equally the remainder. In period 2 the proposer behaves just as in the bargaining equilibrium (forms a MWC and invests in the public good optimally given period 1 investment) except for the choice of the coalition partners. If period 1's proposer provided an

[40] In Table 32 of Appendix D we explore this comparison further.

amount of public good at the efficient level, then the proposer in period 2 invites period 1's proposer into the coalition in period 2. Otherwise, the period 2 proposer punishes period 1's proposer by excluding her from the coalition in period 2. Such a strategy can be implemented in our experiment, since the ID numbers of the committee members remain the same within a match. Theoretically, however, such a strategy cannot be supported as a sub-game perfect equilibrium, since the rewards and punishments are not credible. In the period 2 subgame, after a period 1 proposer deviated from *cooperation* (efficient investment in period 1), any other committee member should exclude her from the coalition. Because she is not included in coalitions proposed by others, the punished agent's continuation value is the lowest of all agents. Thus, any proposer is tempted to deviate, pay her less than the alternative, and include her in the coalition.

To see whether the described punishments/rewards mechanism is consistent with the behavior of subjects that opted to use the EM strategy, we focus on period 2 proposals from subjects who were *not* proposers in period 1. Let x_2^{P1} be the private allocation in period 2 to a subject who was the proposer in period 1 (P1). The dummy variable $A1$ takes value 1 if the period 1 proposer proposed an allocation that benefits equally all three members and 0 otherwise. A period 2 proposer that uses the EM strategy punishes the period 1 proposer by allocating her $x_2^{P1} = 0$ whenever $A1 = 0$ and rewards her by $x_2^{P1} > 0$ whenever $A1 = 1$. Therefore, we would expect $E(x_2^{P1}|A1 = 0) < E(x_2^{P1}|A1 = 1)$. In constrast, we would expect to observe no such difference for other types. We will test for this hypothesis by estimating for each treatment:

$$x_2^{P1} = \alpha_0 + \alpha_1 \cdot A1 + \alpha_2 \cdot EM_{\text{strategy}} + \alpha_3 \cdot (A1 \times EM_{\text{strategy}}) + \epsilon,$$

where $\epsilon \sim N(0, \sigma)$ and EM_{strategy} is a dummy variable that takes value 1 if the proposal involving x_2^{P1} is of Type EM (as defined in Section 5.1). We estimate a random effects regression for each treatment separately and report in Table 9 the average private allocations to period 1 proposers depending on their period 1 behavior ($A1$) and the type of the proposal in period 2.[41] Notice that for proposals that are not of EM type ($EM_{\text{strategy}} = 0$) there is no quantitative difference between proposers who cooperated ($A1$=1) and those who did not ($A1 = 0$). The difference is dictated by the estimates of α_2, which are not significant for DB treatments and, although significant, it is relatively small in the SB case. This is no longer the case for Type EM proposals. Differences are significant in the DB treatments, but of similar magnitude in all cases, between 10% and 15% of the budget approximately.

The previous evidence shows that subjects using EM strategies offer lower payoffs to period 1 proposers who deviated from cooperation, but they still offer a positive amount on average while the theory predicts a payoff of zero. To inspect this further Figure 2 presents the distribution of x_2^{P1} for DB treatments. Consider the top left graphs summarizing the information for EM strategies in the DB^{low} treatment. When the proposer did not cooperate in period 1 ($A1 = 0$) the mode involves zero private allocations. In contrast, when the proposer cooperated in period 1 ($A1 = 1$), there is

[41] The estimated coefficients from equation above are reported in Table 33 of Appendix D.

Table 9: Punishments: Private allocation to period 1 proposer (x_2^{P1})

Strategy type	Period 1 Proposer	SB	DB$^{\text{low}}$	DB$^{\text{high}}$
$EM_{\text{strategy}} = 1$	$A1 = 1$	21.66	27.78	29.05
	$A1 = 0$	14.14	10.45	19.55
$EM_{\text{strategy}} = 0$	$A1 = 1$	23.10	21.31	19.70
	$A1 = 0$	20.50	19.52	22.29

a large mass with positive private allocations. The same qualitative finding holds for EM proposals in the DB$^{\text{high}}$ treatment. This pattern is no longer observed if we focus on MM proposals in both dynamic treatments (second row). The mass of zero offers does not show significant differences depending on the behavior of period 1's proposer ($A1 = 0$ versus $A1 = 1$).

6 Conclusion

When there are dynamic linkages in the inter-temporal provision of a durable public good the usual static inefficiencies are present, but new dynamic inefficiencies arise. Since introducing a dynamic link in a model is typically more theoretically demanding, a prominent question is whether such inefficiencies are empirically meaningful. In this paper, we design an experiment that can isolate static and dynamic inefficiencies in a two-period laboratory environment, and report the findings of that experiment.

When the theory predicts dynamic inefficiencies to be large, the data are in line with the prediction. Our data indicate that subjects respond to the incentives in the environment in similar ways to earlier experiments on legislative bargaining. With experience, bargaining delays become infrequent and minimum winning coalition proposals become more prevalent. The main focus of our analysis is on period 1 investment behavior, because that is key to identifying dynamic inefficiencies and measuring the extent to which they affect outcomes. On average, investment in the first period is highest when the proportion of period 1 investment that survives in period 2 is high, i.e, the depreciation rate of the durable public good is low. Moreover, period 1 investment is monotonically lower as the depreciation rate increases. Accordingly, when dynamic linkages are relatively low, dynamic inefficiencies become less important, as predicted by the theory. Furthermore, we identify two sources of dynamic inefficiencies, the durability effect and the crowding-out effect, and estimate their magnitudes. Our data indicates that, consistent with the theory predictions, the durability effect is significantly larger than the crowding-out effect; however, the magnitudes of both effects are lower than predicted by theory. Overall our data indicates that dynamic inefficiencies can be empirically quite large, especially when depreciation rates are low.

We also document heterogeneity in individual behavior. Some subjects propose investment levels that are close to the planner's in both periods, especially in early matches. While those subjects may be driven by altruism, the prevalence of this behavior is substantially reduced as the session evolves. In later matches we are able to identify two canonical types of behavior that capture most of the data. First, a large proportion of subjects use strategies that involve minimum winning coalitions

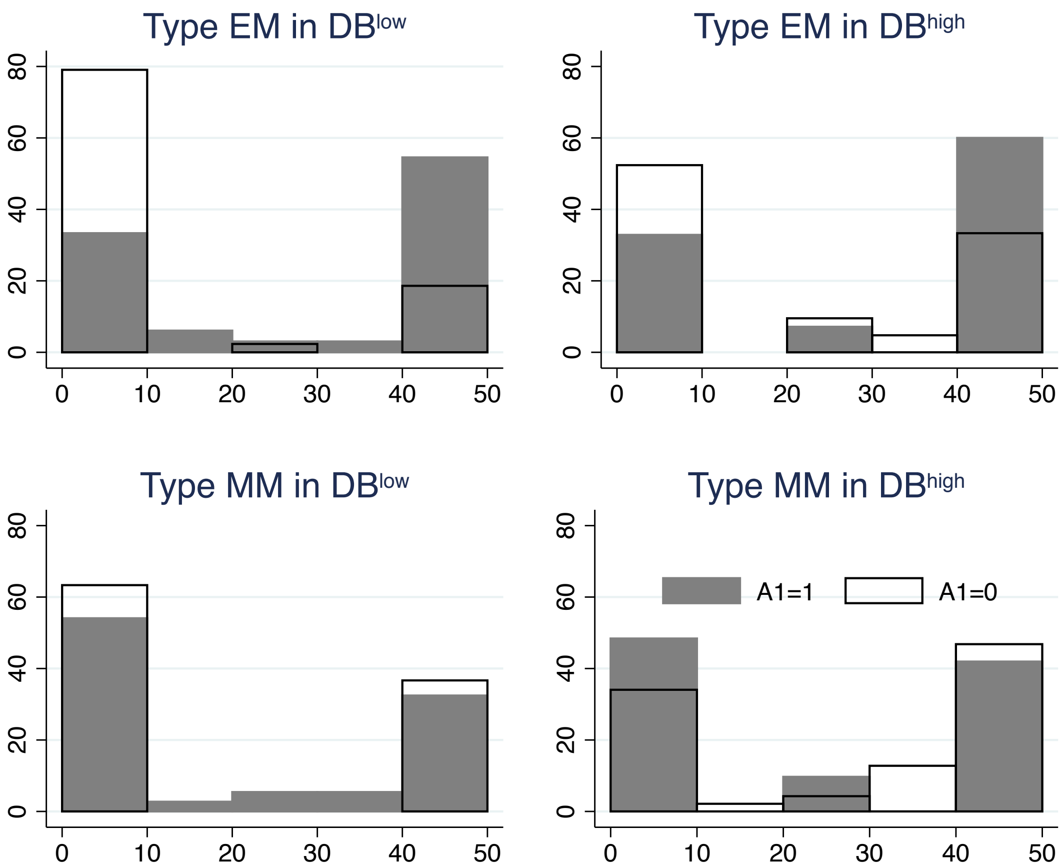

Figure 2: Distribution of Private period 2 allocations to period 1 proposers as % of Budget

(Not) All included: Period 1 proposal (does not) includes all members

Note: This Figure includes period 2 proposals from subjects who were not proposers in period 1 and display investment levels that are, on average, close to the theoretical predictions. The second prevalent behavior involves strategic cooperation. These are subjects whose proposals in the first period are significantly higher than the theoretical level, and then use a minimum winning coalition in period 2 to reward/punish period 1 choices. According to such behavior, if the investment level proposed in period 1 was efficient (or nearly efficient), then the committee member who made that proposal would be invited into the coalition in period 2 and excluded otherwise. While this behavior is not sub-game perfect, average payoffs are quite close to those who always proposed a minimum winning coalition. This behavior is suggestive that a non non-negligible fraction of subjects condition period 2 behavior on period 1 outcomes.

We close with two comments about the two-period approach we use to study dynamic free riding. First, while much of the theory about the dynamic provision of durable public goods has been studied using infinite horizon models, the basic phenomenon of dynamic free riding appears even in simple finite horizon models with two periods of public good accumulation. This allows for much simpler experimental designs, compared with experiments that are designed to mimic an infinite horizon using random termination rules. Methodologically, it allows for a more straightforward analysis of the data, which always appear in blocks of two periods, rather than the data from experiments with random termination rules, where different observations have different numbers of periods. On the other hand, the two period models lack the elegance of the infinite horizon models and cannot address deeper theoretical issues about convergence to stationary states (public good levels) and Markov equilibrium. The philosophy behind our design is that this tradeoff probably favors the two-period model if the goal of the experiment is to compare treatments aimed at sorting out and identifying static and dynamic free riding effects. In other contexts, where the goal is to evaluate outcomes relative the predicted long run steady states or to test for Markov equilibrium, the tradeoff tilts the other way, and there are several examples of this. See, for example Battaglini and Palfrey (2012), Battaglini et al. (2012), Vespa (2015), and Battaglini et al. (2013).

Second, there are other dynamic environments that have been modeled theoretically using infinite horizon stochastic games, which may also merit study in the laboratory using a two-period approach. An example of this is the experimental study Battaglini et al. (2014) of a simple two period version of the Battaglini-Coate infinite horizon model of the political economy of debt and public good provision Battaglini and Coate (2008).

The study of dynamic games in the lab is still relatively new. Although questions having to do with public goods or legislative bargaining have a long history in experimental economics, how this knowledge can be translated to dynamic environments is not self-evident. We think that a richer and more nuanced understanding of the impact of dynamic linkages on these environments can be obtained by both studying infinitely repeated games as well as simpler games that allow to better focus on certain features of the dynamic environment. In the context of legislative bargaining with durable public goods we show that indeed subjects react to the tensions identified by the model, in particular both the force of static and dynamic free-ridding. However, we also find that many subjects deviate from the equilibrium strategy in favor of an efficient strategy with an easy to implement punishment.

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Appendix A: Proofs

The Efficient Solution

The maximization problem specified in section 2.1 can be re-written as

$$\max_{I_1^P, I_2^P} \left[B_1 - I_1^P + n \cdot u\left(I_1^P\right) + B_2 - I_2^P + n \cdot u\left(\delta I_1^P + I_2^P\right)\right]$$
$$\text{s.t. } 0 \le I_1^P \le B_1 \text{ and } 0 \le I_2^P \le B_2$$

There are several cases to deal with depending on which, if any, constraints are binding. If no constraints are binding, then there is an interior solution, $(I_1^{P^*}, I_2^{P^*})$ characterized by two first order conditions:

$$\begin{aligned} u'(\delta I_1^{P^*} + I_2^{P^*}) &= \frac{1}{n} \\ u'(I_1^{P^*}) + \frac{\delta}{n} &= \frac{1}{n} \end{aligned} \tag{1}$$

If the solution is not interior, there are several ways the constraints can be binding. One possibility is that $I_1^P \le B_1$ is binding. A second possibility is that $0 \le I_2^P$ is binding. A third possibility is that $I_2^P \le B_2$ binds, but this is an uninteresting case and in the rest of the paper we assume it never binds. Notice that the constraint $0 \le I_1^P$ is never binding because of the Inada condition on u. The constraint $0 \le I_2^P$ is binding when the value of $I_1^{P^*}$ that solves (1) is such that $u'(\delta I_1^{P^*}) < \frac{1}{n}$, which happens if δ is sufficiently large. In this case, as long as $I_1^P \le B_1$ is not also binding, the solution is given by:

$$u'(I_1^{P^*}) + \delta u'(\delta I_1^{P^*}) = \frac{1}{n} \tag{2}$$

the second equation of (1), with $I_2^{P^*} = 0$. If both $I_1^P \le B_1$ and $0 \le I_2^P$ bind, then the solution is $I_1^{P^*} = B_1$, $I_2^{P^*} = 0$. If only $I_1^P \le B_1$ is binding, then the solution is given by the first equation of (1), with $I_1^{P^*} = B_1$. Finally, observe that our assumptions on u implies a unique planner solution $(I_1^{P^*}, I_2^{P^*})$.

In our experiments, the utility of the public good is given by $u(g) = Ag^\alpha$ and parameters B_1 and B_2 are large enough so that the budget constraints are not binding. Thus, the efficient solution depends on the value of δ:

$$\text{if } \delta^{1-\alpha} < 1 - \delta \text{ then } \begin{cases} I_1^{P^*} = \left[\dfrac{1-\delta}{nA\alpha}\right]^{\frac{1}{\alpha-1}} \\ I_2^{P^*} = \left[\dfrac{1}{nA\alpha}\right]^{\frac{1}{\alpha-1}} - \delta\left[\dfrac{1-\delta}{nA\alpha}\right]^{\frac{1}{\alpha-1}} \end{cases}$$

$$\text{if } \delta^{1-\alpha} \geq 1-\delta \text{ then } \begin{cases} I_1^{P*} = \left[\dfrac{1}{nA\alpha(1+\delta^{\alpha})}\right]^{\frac{1}{\alpha-1}} \\ I_2^{P*} = 0 \end{cases}$$

The Bargaining Equilibrium

Period 2

The randomly chosen proposer only needs to gain the support of $\frac{n-1}{2}$ other members of the committee. Denote by x_2^{Pr} the private allocation the proposer will keep for herself and by x_2^{C} the amount she will give to $\frac{n-1}{2}$ non-proposer committee members 'in her coalition'. Then, her maximization problem is given by:

$$\max_{(x_2^{\mathrm{Pr}}, x_2^{\mathrm{C}}, I_2^L)} \left[x_2^{\mathrm{Pr}} + u(g_2)\right]$$

$$\text{s.t.} \begin{cases} x_2^{\mathrm{Pr}} + \frac{n-1}{2} \cdot x_2^{\mathrm{C}} + I_2^L \leq B_2 \\ x_2^{\mathrm{C}} + u(g_2) \geq V_2(I_1) \\ 0 \leq I_2^L, 0 \leq x_2^{\mathrm{Pr}}, 0 \leq x_2^{\mathrm{C}} \\ g_2 = \delta I_1^L + I_2^L \end{cases}$$

where I_1^L is the level of public good implemented in period 1 of the legislative bargaining game, $V_2(I_1^L)$ is the value of the game in the second period, as a function of I_1^L, before a proposer has been selected. The first constraint involves re-writing the budget constraint using the symmetry assumption. In other words, the private allocation for the proposer plus an equal amount assigned to each other member of a minimum winning coalition (MWC) cannot be higher than the available funds after investment ($B_2 - I_2^L$), where subscript L stands for the legislature. The second constraint guarantees the participation of other coalition members. A non-proposer who is included in the coalition will vote in favor of the proposal if the utility he gets from it (LHS) is at least as high as the equilibrium expected value of rejecting it (V_2). The remaining constraints are feasibility constraints. Being a strictly concave problem, there will be a unique solution for the equilibrium period 2 investment level, I_2^L. Assuming an interior solution, $0 \leq I_2^L \leq B_2$, it is characterized by:

$$u'(\delta I_1^L + I_2^L) = \frac{2}{n+1} \tag{3}$$

In period 2, the proposer weighs the marginal benefit to the public good *to the coalition of* $\frac{n+1}{2}$ *voters* against the marginal cost in units of private good of investing an extra unit in the public good.

FOC (3) captures the optimal period 2 investment in the bargaining game as a function of the investment in the first period, I_1. As in the analysis of the planner's solution, it is possible that $u'(\delta I_1^L) < \frac{2}{n+1}$, in which case (3) violates the constraint $0 \leq I_2^L$. Thus, the full characterization of how I_2^L varies as a function of I_1^L is the following:

$$I_2^L(I_1^L) = \begin{cases} u'^{-1}\left[\dfrac{2}{n+1}\right] - \delta I_1^L \text{ if } u'(\delta I_1^L) \geq \dfrac{2}{n+1} \\ 0 \text{ otherwise} \end{cases}$$

The funds remaining once the unique optimal investment level has been determined are simply $B_2 - I_2^L(I_1^L)$. These remaining funds will be allocated among committee members just as in a Baron-Ferejohn multilateral bargaining game with no public good, giving:

$$\begin{aligned} x_2^{\mathrm{C}} &= \frac{1}{n}\left(B_2 - I_2^L(I_1)\right) \\ x_2^{\mathrm{Pr}} &= \frac{n+1}{2n}\left(B_2 - I_2^L(I_1)\right) \end{aligned}$$

Finally, we can also use the equilibrium levels of these allocations to compute the equilibrium continuation value in period 2, $V_2(I_1)$:

$$V_2(I_1^L) = \frac{1}{n}\left(B_2 - I_2^L(I_1^L)\right) + u(\delta I_1^L + I_2^L(I_1)) \tag{4}$$

Period 1

The selected period 1 proposer anticipates how her decisions will impact choices in period 2. The maximization problem of the proposer in period 1 can be written as:

$$\max_{(x_1^{\mathrm{Pr}}, x_1^{\mathrm{C}}, I_1^L)} \left[x_1^{\mathrm{Pr}} + u(I_1^L) + V_2(I_1^L)\right]$$

$$\text{s.t.} \begin{cases} x_1^{\mathrm{Pr}} + \frac{n-1}{2} x_1^{\mathrm{C}} + I_1^L \leq B_1 \\ x_1^{\mathrm{C}} + u(I_1^L) + V_2(I_1^L) \geq V_1 \\ 0 \leq I_1^L \leq B_1, 0 \leq x_1^{\mathrm{Pr}}, 0 \leq x_1^{\mathrm{C}} \end{cases} .$$

where V_1 is the expected value of the game to each player before a proposer has been selected. The function to maximize includes the proposer's period 1 utility and the equilibrium expected value of the game for period 2, which depends on I_1^L. Maximization is constrained by the budget and by the fact that any coalition member expects the proposal to provide at least as much as he would receive by rejecting it (V_1). The first constraint obviously holds with equality. If the second constraint is binding and the feasibility constraints are not binding (i.e., the solution is interior), then the maximization problem for the proposer in period 1 can be rewritten as:

$$\max_{I_1^L} \left[B_1 - I_1^L - \frac{n-1}{2} V_1 + \frac{n+1}{2}\left[u(I_1^L) + V_2(I_1^L)\right]\right]$$

The first order condition that characterizes the equilibrium investment in period 1, I_1^L is:

$$u'(I_1^L) + \frac{dV_2}{dI_1}|_{I_1^L} = \frac{2}{n+1} \tag{5}$$

The left hand side again reflects the distortions from the planner's solution due to a combination of free rider effects (both dynamic and static) and the bargaining advantage of the period 1 proposer. There are two separate dynamic effects, because I_1^L affects V_2 in two ways: there is a direct effect on the level of public good in period 2, which we refer to as the *durability effect*; second, there is an *indirect* effect on the equilibrium private allocations in period 2, which we call the *crowding-out* effect.

$$\frac{dV_2}{dI_1}|_{I_1^L} = -\frac{1}{n}\frac{dI_2^L}{dI_1}|_{I_1^L} + \left[\delta + \frac{dI_2^L}{dI_1}|_{I_1^L}\right] u'\left[\delta I_1^L + I_2^L(I_1^L)\right] \tag{6}$$

Case 1: Interior solution. At an interior solution (i.e., $I_1^L < B_1$ and $I_2^L(I_1^L) > 0$), $\frac{dI_2}{dI_1} = -\delta$, so the first term reduces to $\frac{\delta}{n}$, and the second term vanishes because the increased period 1 investment completely crowds out period 2 investment. Hence in this case, the entire dynamic free riding effect is due to the indirect *crowding-out* effect, that is, $\frac{dV_2}{dI_1}|_{I_1^L} = \frac{\delta}{n}$. Substituting back into the first order condition for the equilibrium period 1 proposal, 5, gives:

$$u'(I_1^L) + \frac{\delta}{n} = \frac{2}{n+1} \tag{7}$$

Thus, the crowding out effect actually *reduces* the free rider problem, since the (interior) value of I_1^L that solves (7) is strictly higher than the solution if $\delta = 0$, and is actually increasing in δ. The intuition behind this is that the period 1 proposer can reduce the side payments to coalition members by increasing V_2 (by freeing up more period 2 budget for private allocations), and raises her own payoff at the same time.

Case 2: Corner solution, $I_2^L(I_1^L) = 0$. If in equilibrium the constraint $0 \leq I_2$ binds, then $\frac{dI_2}{dI_1} = 0$, and the first term vanishes. In this case, investment in period 1 will not substitute for investment in period 2 at the margin. Hence in this case, the entire dynamic free riding effect is due to the direct *durability* effect. That is, $\frac{dV_2}{dI_1}|_{I_1^L} = \delta u'(\delta I_1)$. Substituting back into the first order condition for the equilibrium period 1 proposal, 5, gives:

$$u'(I_1^L) + \delta u'(\delta I_1^L) = \frac{2}{n+1} \tag{8}$$

Given functional form used in our experiments $u(g) = Ag^\alpha$, the equilibrium investment levels in the legislative bargaining game are given by:

$$\text{if } \delta^{1-\alpha} < 1-\delta\frac{n+1}{2n} \text{ then } \begin{cases} I_1^{L^*} = \left[\dfrac{1-\frac{n+1}{2n}\delta}{\frac{n+1}{2}A\alpha}\right]^{\frac{1}{\alpha-1}} \\ I_2^{L^*} = \left[\dfrac{1}{\frac{n+1}{2}A\alpha}\right]^{\frac{1}{\alpha-1}} - \delta\left[\dfrac{1-\frac{n+1}{2n}\delta}{\frac{n+1}{2}A\alpha}\right]^{\frac{1}{\alpha-1}} \end{cases}$$

$$\text{if } \delta^{1-\alpha} \geq 1-\delta\frac{n+1}{2n} \text{ then } \begin{cases} I_1^{L^*} = \left[\dfrac{1}{\frac{n+1}{2}A\alpha(1+\delta^{\alpha})}\right]^{\frac{1}{\alpha-1}} \\ I_2^{L^*} = 0 \end{cases}$$

Finally, we note that it is straightforward generalizes analysis presented here to any quota voting rule, where a proposal passes if a winning coalition requires at least q individuals, where q is any integer from 1 to n. The main idea is that the free riding problem is linked directly the fact that a proposer will only internalize the value to q members of the legislature, since that is all she needs for the proposal to pass. When $q = n$, there is no free rider problem, and the optimal public investment is the equilibrium investment.

Appendix B: Instructions

Below we present the instructions for DB^{high} treatment. Periods are referred to as Rounds.

Written instructions

Welcome

You are about to participate in an experiment on decision making and you will be paid for your participation with cash vouchers, privately at the end of the session. The currency in this experiment is called tokens. All payoffs are denominated in this currency. Tokens that you earn in the experiment will be converted into US dollars using the rate 10 Tokens = \$1. In addition, you will get \$10 participation fee if you complete the experiment. The money you earn will depend on your decisions, the decisions of others and chance.

Do not talk to or attempt to communicate with other participants during the session. Please make sure to turn off phones, mp3 players and pagers now. The session will begin with a brief instructional period, during which you will be informed of the main features of the task and you will be shown how to use the computer.

Basic Steps

In this experiment you will act as voters that distribute funds between yourself and others in a series of matches. Each match consists of two rounds. In each round you must decide on how to split a sum of money between yourself, two others and a group project. Proposals will be voted up or down (accepted or rejected) by majority rule; i.e., for proposals to pass they must get 2 or more votes.

Each match starts with round 1. Your three-member group will have to decide how to divide 200 tokens. To do this each member of the group will submit a proposal that specifies how the 200 tokens are divided between you, the two other voters and the group project.

After you have all made your proposals, one of them will be selected at random to be voted on. All proposals have equal probability of being selected. The proposed allocation will be posted on your computer screens and you will have to decide whether to accept or reject it.

- If the proposed allocation passes (gets 2 or more votes) – it is binding and you move on to the round 2.
- If the proposal is defeated (gets less than 2 votes), there will be a call for new proposals. This process will repeat itself until a proposed allocation passes (gets 2 or more votes).

In round 2, the group will again have to allocate 200 tokens between you, two other voters and the group project. The process is the same: each member of the group starts by submitting a proposal.

The difference with the previous round is that part of what the group allocated to the group project in round 1 is still available in round 2. In other words, the project size at the beginning of round 2 is 80% of the amount invested in the group project in round 1. For example, if the group project that passed in round 1 was 15 tokens, then the project size at the beginning of round 2 is 12 tokens. The total project size at the end of round 2 will be the project size at the beginning of round 2 (which is 12 tokens in our example) plus the investment in the group project in round 2.

After you have all made your proposals, one of the proposed allocations will be selected at random to be voted on and you will have to decide whether to accept or reject it.

- If the proposed allocation passes (gets 2 or more votes) – it is binding and you move on to round 1 in a new Match.
- If the proposal is defeated (gets less than 2 votes), there will be a call for new proposals and the process will repeat itself until a proposed allocation passes (gets 2 or more votes).

Payoffs

Your Payoff in round 1 depends only on the proposed allocation that passed:

$$\begin{array}{c}\text{Your}\\\text{Payoff}\\\text{in round 1}\end{array} = \begin{array}{c}\text{Your}\\\text{Individual Allocation}\\\text{in round 1}\end{array} + 5\times\left(\begin{array}{c}\text{Investment}\\\text{in Group Project}\\\text{in round 1}\end{array}\right)^{0.5}$$

Your Payoff in round 2 depends on the proposed allocation that passed and on the size of the group project at the beginning of round 2.

$$\begin{array}{c}\text{Your}\\\text{Payoff}\\\text{in round 2}\end{array} = \begin{array}{c}\text{Your}\\\text{Individual Allocation}\\\text{in round 2}\end{array} + 5\times\left(\begin{array}{c}\text{Project Size at the beginning of round 2}\\+\\\text{Investment in Group Project in round 2}\end{array}\right)^{0.5}$$

When you are considering what proposal to submit, the computer interface will let you compute the payoffs implied by your proposal for each voter of the group.

Number of matches

In this session there will be total of 10 matches with two rounds in each match. Before the beginning of each match, participants in the experiment will be divided randomly to the groups of 3 voters. The identity of your group members will never be revealed to you and your group members will never know your identity. You will stay in the same group in both rounds of a match. Once the second round of the match is over, you will be randomly allocated to a new group of 3 voters.

How tokens are converted into cash payments

At the conclusion of the experiment, one of the 10 matches played for tokens will be randomly selected by computer, and the tokens you earned in this match (both in the first and in the second round) will be converted into US dollars using a conversion rate of 10 Tokens = \$1. In addition you will receive the \$8 participation fee for completing the experiment.

To summarize

- The experiment consists of 10 matches. In each match participants are assigned to groups of 3 voters in each. Each match consists of two rounds.
- In each round all members of the group submit a proposal to allocate 200 tokens between a group project and individual allocations to each of the 3 voters. One of these proposals is selected at random and is voted on by all group members. All proposals have equal chance of being selected.
- Tokens allocated to the group project yield positive payoffs for all group members, while tokens allocated to the individual members of the group benefit only those members.
- The allocation passes if two or more voters accept it. If the allocation passes in round 1, the group moves on to round 2.
- If the allocation passes in round 2, a new match will begin. If the allocation is rejected, then there will be a call for new proposals.
- Your payoff in round 1 depends on your individual allocation in round 1 and the group investment in round 1.
- The project size at the beginning of round 2 is 80% of the investment in the Group Project in round 1.
- Your payoff in round 2 depends on your individual allocation in round 2, the investment in the Group Project in round 2 and the size of the Group Project at the beginning of round 2.
- Total payoff in the match = Payoff in round 1 + Payoff in round 2.

- Once the match is over, participants are randomly assigned to new groups of 3 voters in each and the next match begins, which is identical to the previous one.
- At the end of the experiment, one match is chosen randomly by the computer, and the tokens earned in this match are converted into US dollars, added to the participation fee and paid to participants in cash vouchers.

Difference in Instructions for Static and Dynamic treatments

The difference between instructions in the SB treatment and presented above DB^{high} treatment was the part describing that some portion of the public investment from round 1 survives to round 2 in the DB^{high} treatment, while it is not in the SB treatment. In particular, in the SB treatment the instructions did not contain the paragraph "The difference with the previous round is that part of what the group allocated to the group project in round 1 is still available in round 2."

In addition, payoffs for round 2 were described as follows:

Your Payoff in round 2 also depends only on the proposed allocation that passed:

$$\begin{array}{c}\text{Your}\\\text{Payoff}\\\text{in round 2}\end{array} = \begin{array}{c}\text{Your}\\\text{Individual Allocation}\\\text{in round 2}\end{array} +5\times\left(\begin{array}{c}\text{Investment}\\\text{in Group Project}\\\text{in round 2}\end{array}\right)^{0.5}$$

Script and Slides

This script was read aloud while projecting some slides. See Figure 3 to follow most instructions. Comments between brackets were not read aloud.

Script

We will now conduct a practice round that will not count for money. As we move on this practice round please do not click or enter any information until I ask you to. If you have any questions raise your hand.

The experiment will take place over a sequence of 10 matches. We begin the match by dividing you into committees of three members each. Each of you is assigned to exactly one of these committees. In each match your committee will make budget decisions by majority over a sequence of two rounds.

[SHOW SLIDE A1+A2+A3+P=200]

[point while reading] In each round your committee has a budget of 200. Your committee must decide how to divide this budget into four categories, in integer amounts: the first three categories are the private allocations and they always have to be greater than or equal to 0. The fourth category is for investment in a project and it also must be greater than or equal to 0.

If your committee's budget decision is (A1, A2, A3, P), then A1 go directly to member 1's earnings, A2 to member 2 and A3 to member 3. The project investment produces earnings for all committee members in the following way.

The project earnings in a round depend on the size of the project at the end of that round. Specifically, each committee member earns an amount in points proportional to the square root of the size of the project at the end of the round (precisely equal to 5*sqrt(project size)). During the experiment, there will be a graph on the screen that shows exactly how project earnings will depend on project size.

[SHOW SLIDE GRAPH]

For example, if the size of the project at the end of the round equals 121, then each member earns exactly 5*sqrt(121) or 55 additional points in that round. If the size is equal to 25, each member earns exactly 5*sqrt(25) or 25 additional points in that round. In your display, earnings are always rounded to two decimal places. So, for example if the project size at the end of a round equals 70, each member earns 41.83 points from the project in that round.

As we said before, there are two rounds. The project size at the end of round 1 is simply what the committee allocated to the project in round 1. 80% of the amount invested in the project in round 1 carries over to round 2, so project size at the beginning of round 2 is 80% of project size in round 1. Finally project size at the end of round 2 is the project size at the beginning of round 2 plus the investment in the project in round 2.

At the end of each round your earnings for that round are computed by adding the project earnings to your private allocation. For example, if your private allocation is 20 and the end-of-round project size is 121, then your earnings for that round equal 20 + 5*sqrt(121) = 20+5*11 = 75. Your earnings for the match equal the sum of the earnings in both rounds of that match.

After the first match ends, we move to match 2. In this new match, you are reshuffled randomly into new committees of three members each. The match then proceeds the same way as match 1.

We will now go through one practice match very slowly. During the practice match, please do not hit any keys until I tell you, and when you are prompted by the computer to enter information, please wait for me to tell you exactly what to enter. You are not paid for this practice match.

[AUTHENTICATE CLIENTS-Start Multistage]

Please double click on the icon on your desktop that says BP2. When the computer prompts you for your name, type your First and Last name. Then click SUBMIT and wait for further instructions.

[accept and start game] [screenshot]

You now see the first screen of the experiment on your computer. It should look similar to this screen. [POINT]

You have been assigned by the computer to a committee of three subjects, and assigned a committee member number: 1, 2 or 3. This committee assignment and your member number stay the same for both rounds of this match, but will change across matches. It is very important that you take careful note of your committee member number.

Your committee decides on a budget for this round by the following voting procedure. First, every member is asked to type in a provisional budget proposal, consisting of four integers, A1, A2, A3 and P, which add up to 200. A1, A2, A3 and P have to be greater than or equal to 0.

Ok. In the example... committee member # individual allocations should be entered here project investment here. As we proceed note that any information pertaining to you specifically will be in red.

[point to graph as appropriate, while reading this]

As a visual aid, there is a graph on the left that shows exactly how project earnings will depend on project size. The current size of the project is marked with a large dot. If your committee decides to invest nothing this period, then this will be the size that determines your project earnings at the end of the round. You can use your mouse to move the cursor along the curve to figure out what your earnings will be for different levels of investment. Also, if you type in a budget amount in the Project box, the computer will compute and display the corresponding project earnings for you just below the box.

Take a minute to practice using your cursor to move along the curve, and typing in different possible investment levels for the Project. But do not hit the confirm button yet.

[wait one minute]

At this time, go ahead and type in any provisional proposal you wish and hit the confirm button. You are not paid for this practice match so it does not matter what you enter.

[wait for responses] [screenshot]

After everyone in your committee has submitted a provisional budget proposal, your screen should now look similar to this one [POINT]. The computer has randomly selected one of the provisional budget proposal submitted by the members of your committee to be the Round One Proposed Budget in your committee. In the top-right of your screen you are shown this proposed budget as well as which committee member made this proposal. [POINT]

[In this example Again: member Number Current project size Proposer Proposed budget. These numbers are random Note that when information is displayed in a quadruple, it is always listed in order of memberNumber. So... Also note : I am member 3, so the number is red.] At this moment all committee members are asked to vote on the Proposed Budget. The decision is made by majority rule. The Proposed Budget passes if it receives 2 or 3 votes. Otherwise, it fails, there will be a call for new proposals and the process will repeat itself until a proposed budget passes (gets 2 or 3 votes). To vote to accept the Proposed Budget, click on the "yes" button; to reject it, click on the "no" button. Please go ahead and vote "yes" now. Since this is a practice round that doesn't count for money please all click on "yes" button.

[wait for responses] [screenshot]

[point] In addition to your committee member number, you can see each member's vote, the outcome of the vote, and the end of round project size. You can also see your earnings in round 1 and the project size in the beginning of round 2.

[In this example Again, in order: memberNumber Votes Outcome End-Of-Round project size]

This marks the end of the round.

The table with columns in the bottom of your screen is the History panel and summarizes all of this important information.

[Go BRIEFLY over history panel]

[click to advance to next round] [screenshot]

Now the second round begins. In this second round, you keep the same committee member number as in the first round, and the members of your committee all stay the same. Notice that 80% of the project investment from round 1 carries over, so the round 2 beginning project size equals 0.8 times the project size at the end of round 1.

[In this example Project size upper right hand corner Project size at origin of graph]

In this second round each member of the committee is asked to submit Provisional Budget Proposal of how to divide 200 between yourself, two other committee members and project investment. Please enter your Provisional Budget Proposal now.

[screenshot]

One of the proposals was randomly chosen to be voted on. [In this example proposal]

On the graph, you can see the project size if this proposal will pass. At this time all committee members are asked to vote on the chosen budget proposal. If two or three members of the committee vote yes, then the proposal will pass and this will be the end of the match. If the proposal will fail then there will be a call for new proposals and the process repeats itself until the proposal is passed. You can now finish this round, by voting "yes" on the budget proposal.

[screenshot] [wait for them to finish]

Finally on this screen you see the proposal that passed and your earnings for this round.

Now we are ready for the comprehension quiz. Everyone must answer all the questions correctly before we go to the paid matches. The quiz has four pages. You must answer all the questions on Page 1 of the quiz to proceed to Page 2, and you must answer all the questions on Page 2 of the quiz to proceed to Page 3, etc.... If you answer any of the questions on a page incorrectly, you will be asked to try again. Please raise your hand if you have any questions during the quiz, and we will come to your desk and answer your question in private.

[reassure them its ok to ask for help]

[Quiz detailed below]

[WAIT FOR END OF QUIZ]

Are there any questions before we begin with the paid session?

[WAIT FOR QUESTIONS]

We will now begin with the first of 10 paid matches of the experiment. If there are any problems or questions from this point on, raise your hand and an experimenter will come and assist you in private.

Quiz

Handout in the instructions

[Not read: Extra handout to subjects that the experimenter reads with the subjects]

Before we start with the experiment we ask you to answer a few questions related to the instructions. You can make up to $4 if you answer these questions correctly. The table below is provided to familiarize you with the payoffs and help you when you answer the questions. The first column shows possible allocations to the Group Project in round 1 from 200 to 0 with changes of 10 tokens. The second column displays how the investment in the group project will be transformed into tokens for payoffs. The third column shows what will be available for individual allocations in round 1. The fourth column shows the part of the round 1 Investment in the Group Project that will be available in round 2: The Size of the Project at the beginning of round 2. The last column displays the portion of round 2 payoffs that comes from the Project Size at the beginning of round 2.

Investment in Group Project in Round 1	Round 1 PAYOFF coming from GROUP PROJECT	Round 1 Budget remaining for Individual Allocations	Group Project Size of Project at the Beginning of Round 2	Round 2 PAYOFF coming from Investment in Round 1
	$5\times\left(\text{Investment in Group Project in Round 1}\right)^{0.5}$	$200-\left(\text{Investment in Group Project in Round 1}\right)$	$80\%\times\left(\text{Investment in Group Project in Round 1}\right)$	$5\times\left(\text{Group Project Size at the beginning of Round 2}\right)^{0.5}$
200	70.71	0	160	63.25
190	68.92	10	152	61.64
180	67.08	20	144	60.00
170	65.19	30	136	58.31
160	63.25	40	128	56.57
150	61.24	50	120	54.77
140	59.16	60	112	52.92
130	57.01	70	104	50.99
120	54.77	80	96	48.99
110	52.44	90	88	46.90
100	50.00	100	80	44.72
90	47.43	110	72	42.43
80	44.72	120	64	40.00
70	41.83	130	56	37.42
60	38.73	140	48	34.64
50	35.36	150	40	31.62
40	31.62	160	32	28.28
30	27.39	170	24	24.49
20	22.36	180	16	20.00
10	15.81	190	8	14.14
0	0.00	200	0	0.00

Questions as they appear on the screens

Screen 1

1. For a budget proposal, what do your Private Investments and Project Investment have to add up to? a) 50; b)100; c)200; d) 250

2. There are two rounds in each match. a) True; b) False. There are ten rounds in each match. c)False. The number of rounds in each match depends on the roll of the die.

3. Your committee member number stays the same throughout the experiment. a) True; b) False. Your committee member number stays the same in every round of a match, but is reassigned for new matches.

4. You are reshuffled into a different committee for each match. a) True; b) False. You are in the same committee in all matches.

Screen 2

1. In round 1, assume that in the allocation supported by a majority of committee members the Project Investment is 30 and your individual allocation is 20. How much of your payoff would come from the Project Investment? a) 50; b) 38.73; c) 27.39; d) 65.19
2. How much of your payoff would come from the individual allocation? a) 30; b) 100; c) 20; d) 170
3. What size is the Project Investment at the beginning of round 2? a) 48; b) 24; c) 72; d) 112
4. How much of your payoff in round 2 would come from the Project Investment in round 1? a) 112.25; b) 24.49; c) 58.31; d) 15.81; e) 20

Screen 3

1. In round 1, assume that in the allocation supported by a majority of committee members the Project Investment is 100 and your individual allocation is 20. How much of your payoff would come from the Project Investment? a) 50; b) 38.73; c) 57.01; d) 65.19
2. How much of your payoff would come from the individual allocation? a) 30; b) 100; c) 20; d) 170
3. What size is the Project Investment at the beginning of round 2? a) 72; b) 88; c) 80; d) 40
4. How much of your payoff in round 2 would come from the Project Investment in round 1? a) 44.72; b) 25.50; c) 50.99; d) 22.36

Screen 4

1. In round 1, assume that in the allocation supported by a majority of committee members the Project Investment is 170 and your individual allocation is 20. How much of your payoff would come from the Project Investment? a) 171.03; b) 189.74; c) 206.76; d) 195.58
2. How much of your payoff would come from the individual allocation? a) 30; b) 100; c) 20; d) 170
3. What size is the Project Investment at the beginning of round 2? a) 144; b) 128; c) 136; d) 120
4. How much of your payoff in round 2 would come from the Project Investment in round 1? a) 30; b) 58.31; c) 129.15; d) 60

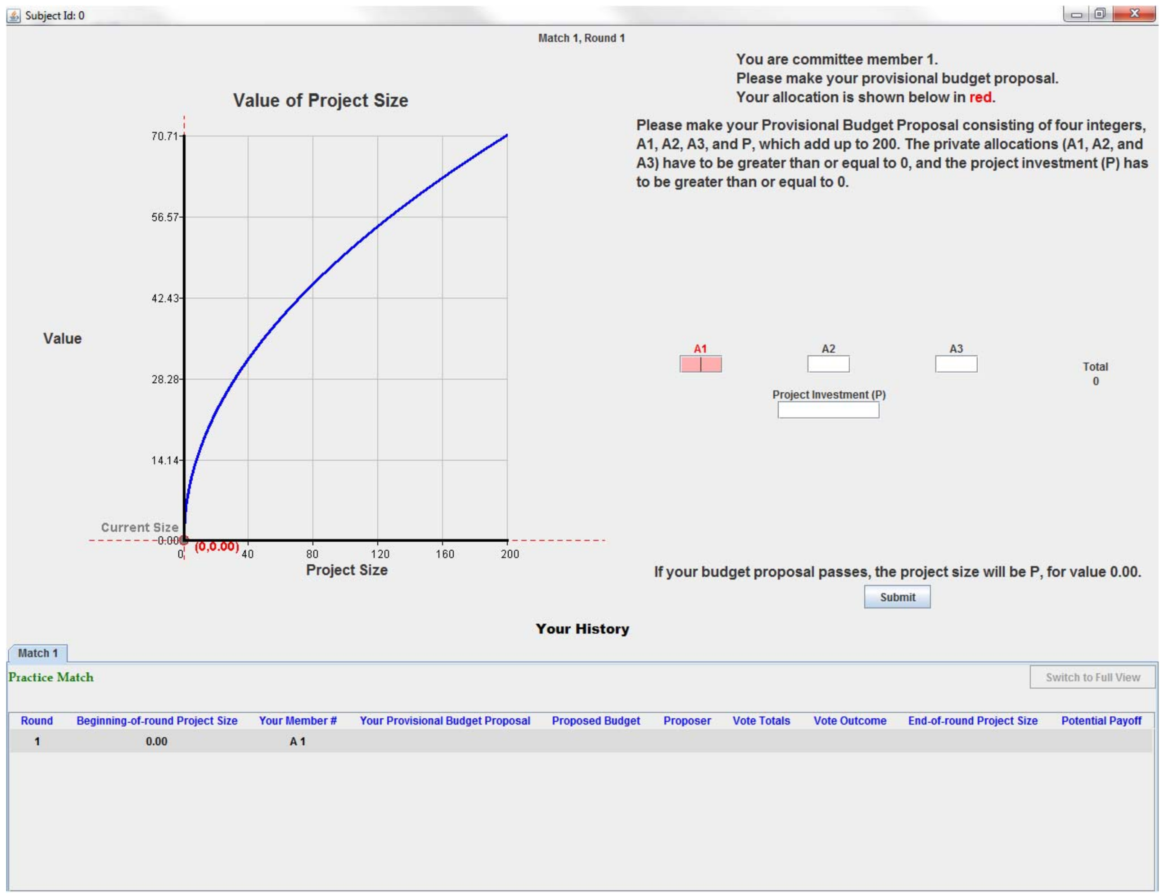

Figure 3: Screen shot

Appendix C: Further Analysis at the Aggregate Level

Number of Stages

The data shows a high proportion of accepted first-stage proposals, similar to levels previously reported in the literature (see for example Fréchette et al. 2003). If we consider the last five matches in all treatments aggregating both periods the percentage of accepted proposals is above 80% and the median number of stages required is one. Figure 4 shows for each period and treatment the average number of stages needed for a proposal to pass as the session evolves.

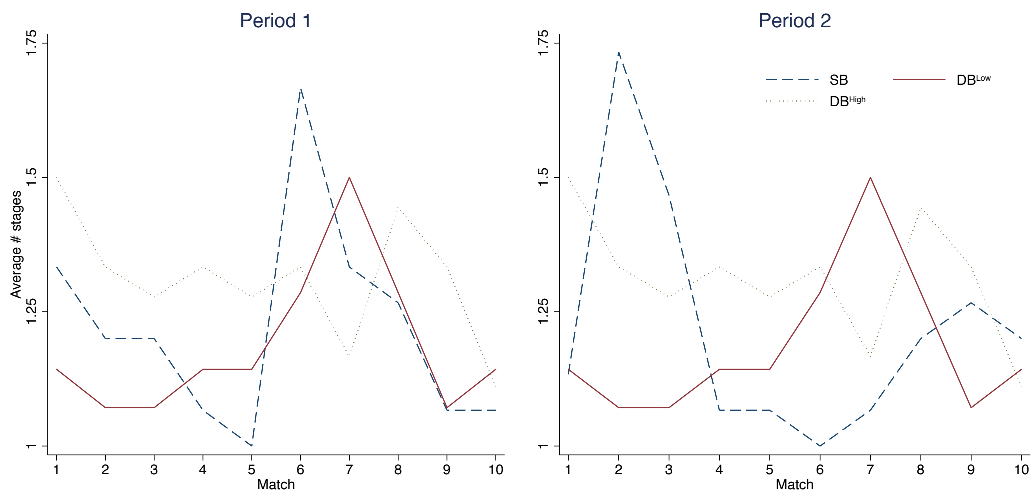

Figure 4: Average Number of Stages

Minimum Winning Coalitions

Table 10 summarizes information with respect to MWCs in our dataset. Several patterns, common to all treatments, emerge from the table. First, the proportion of MWCs is statistically higher in the second period than in the first period.[42]

Second, as subjects gain experience with the environment, they use MWCs more often. With the exception of the DB^{high} treatment (for period 1 proposals), in all cases the number of proposals involving MWC is statistically higher when we focus on the last five matches.[43] Third, almost all subjects proposing a MWC in the first period behave likewise in the second. The corresponding

[42]We run a panel random effects regression with a dummy variable that takes value 1 if the proposal involves a MWC on the left-hand side and a period dummy (1 for the second period) on the right-hand side. We cluster standard errors by session. The p-values on the period dummy are: 0.000, 0.000 and 0.030 for the SB, DB^{low}, and DB^{high} treatments respectively.

[43]We run a panel random effects regression with a dummy variable that takes value 1 if the proposal involves a MWC on the left-hand side and a match dummy (1 for the matches 6-10) on the right-hand side. We cluster standard errors by session. Focusing on period 1 proposals, the p-values on the match dummy are: 0.000, 0.083 and 0.273 for the SB, DB^{low}, and DB^{high} treatments respectively. The corresponding p-values for period 2 proposals are: 0.000, 0.002, and 0.000.

Table 10: Proposals involving Minimum Winning Coalitions (in %)

Treatment		All Matches	Last Five
	Period 1	55.6	66.2
SB	Period 2	67.8	84.4
	Both	51.3	63.6
	Period 1	27.1	32.4
DB^{low}	Period 2	52.1	64.3
	Both	26.2	33.3
	Period 1	33.5	37.4
DB^{high}	Period 2	65.6	78.2
	Both	31.7	36.3

proportion reported in the third row of each treatment captures how many subjects proposed MWCs in both periods and this figure closely follows the one reported for the first period.

There are also differences across treatments. First, when there is full depreciation (SB treatment) period 1 rates are on average higher than compared to other treatments. In the DB treatments only about a third of proposals involve MWCs in the first period, the lowest figures of the table. This difference almost disappears when we compare second period rates. Second, although in some cases the differences are small, rates are slightly higher in the DB^{high} treatment when compared to the DB^{low} treatment.[44]

Period 1 investment

Figure 5 displays the histograms for Period 1 investment across treatments and for different subsets of proposals: all, proposals that passed and proposals involving MWCs. Investment is heavily concentrated around the bargaining equilibrium prediction in the SB case, but is relatively more spread out in DB treatments. In almost every DB case we observe two modes for investment, one relatively close to the bargaining equilibrium and a second closer to the planner's solution. The exception is the DB^{high} case for proposals involving MWCs, where most investment levels are relatively closer to the bargaining equilibrium.

[44]We run a panel random effects regression with a dummy variable that takes value 1 if the period 1 proposal involves a MWC on the left-hand side and a treatment dummy (three pairwise comparisons in three separate regressions) on the right-hand side. We cluster standard errors by session. When the treatment dummy takes value 0 for the SB treatment, the coefficients and p-values for the treatment dummy are: -0.339 and 0.016 (when comparing to DB^{low}), and -0.281 and 0.061 (when comparing to DB^{high}). When the treatment dummy involves DB^{low} (0) and DB^{high} (1), the coefficient is positive (0.057), but not significant (p-value 0.589).

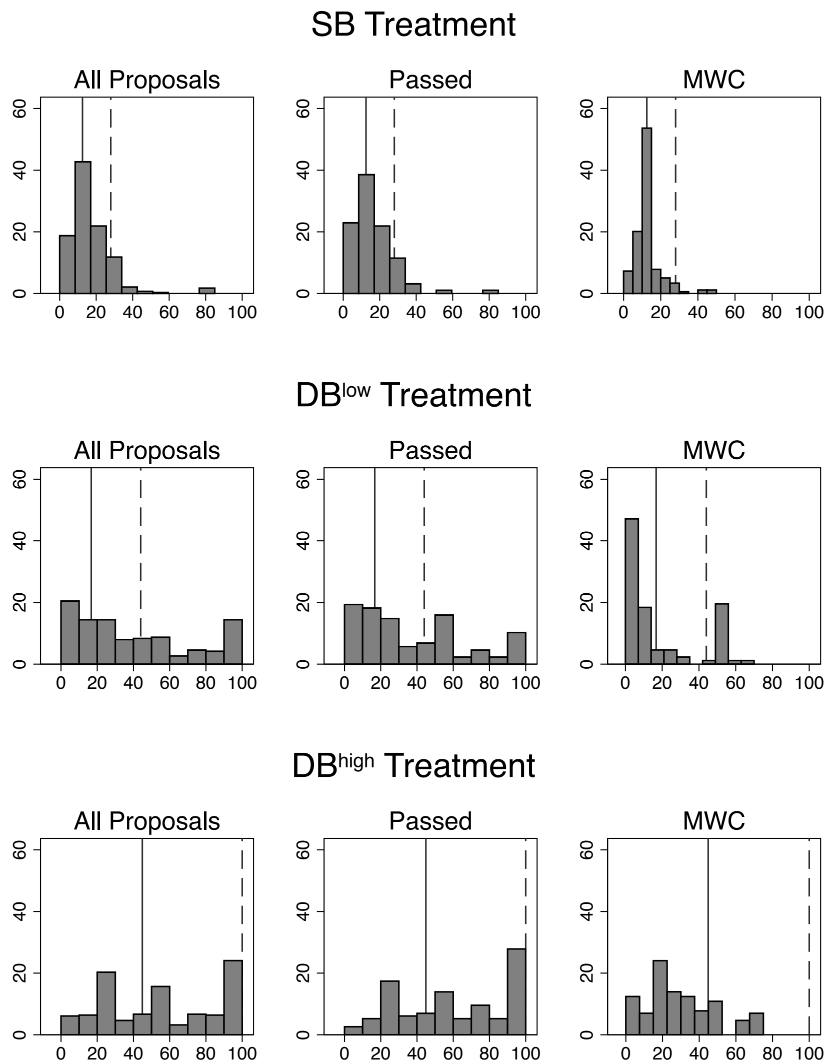

Figure 5: Distribution of Period 1 Investment as % of Budget

Solid lines: Bargaining equilibrium; Dashed lines: Planner's solution

Vertical axis measures the % of proposals in the corresponding bin

Tables with support for statistical tests

Table 11: Difference between period 1 and period 2 investment decisions (last 5 matches)

	SB		DB^{low}		DB^{high}	
	QR	RE	QR	RE	QR	RE
Estimate	0.000	3.813**	10.000***	24.288***	35.000***	41.130***
Std. Err.	0.209	1.565	3.575	4.724	7.651	7.216
p-value	1.000	0.015	0.006	0.000	0.000	0.000

Notes: QR: Quantile regression (estimate of the conditional median), RE: Random Effects Panel Estimation (estimate of the conditional mean). Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Dependent variable: $I_1 - I_2$, Control: Constant

Table 12: Difference between period 1 and period 2 investment decisions (all 10 matches)

	SB		DB^{low}		DB^{high}	
	QR	RE	QR	RE	QR	RE
Estimate	0.000	2.501***	7.500***	21.779***	30.000***	36.019***
Std. Err.	0.000	0.737	2.693	5.084	9.523	5.950
p-value	.	0.001	0.006	0.000	0.002	0.000

Notes: QR: Quantile regression (estimate of the conditional median), RE: Random Effects Panel Estimation (estimate of the conditional mean). Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Dependent variable: $I_1 - I_2$, Control: Constant

Table 13: Quantile regression output for investment decisions (period 1 and period 2)

	SB		DB^{low}		DB^{high}	
	Period 1	Period 2	Period 1	Period 2	Period 1	Period 2
Estimate	10.000***	10.000***	32.500***	5.000	55.000***	0.000
Std. Err.	1.876	0.997	8.102	4.819	7.659	1.012
p-value	0.000	0.000	0.000	0.301	0.000	1.000
p-value(Planner's)	0.000	0.000	0.157	0.003	0.000	1.000
p-value (Leg. Barg.)	0.184	0.013	0.052	0.385	0.188	1.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Planner's): indicates the p-value of a test where the null hypothesis is that the estimate equals the planner's solution
p-value(Leg. Barg.): indicates the p-value of a test where the null hypothesis is that the estimate equals the legislative bargaining solution

Table 14: Quantile regression output for period 1 investment decisions (all proposals and proposals that involve MWCs)

	SB		DB^{low}		DB^{high}	
	All	MWC	All	MWC	All	MWC
Estimate	10.000***	10.000***	32.500***	10.000	55.000***	25.000***
Std. Err.	1.876	0.713	8.102	6.638	7.659	4.865
p-value	0.000	0.000	0.000	0.137	0.000	0.000
p-value(Planner's)	0.000	0.000	0.157	0.000	0.000	0.000
p-value (Leg. Barg.)	0.184	0.001	0.052	0.317	0.188	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Planner's): indicates the p-value of a test where the null hypothesis is that the estimate equals the planner's solution
p-value(Leg. Barg.): indicates the p-value of a test where the null hypothesis is that the estimate equals the legislative bargaining solution

Table 15: Random effects regression output for period 1 investment decisions (all proposals and proposals that involve MWCs)

	SB		DB^{low}		DB^{high}	
	All	MWC	All	MWC	All	MWC
Estimate	16.713***	11.337***	38.712***	18.989***	55.200***	30.566***
Std. Err.	2.867	2.177	6.988	3.803	4.654	3.601
p-value	0.000	0.000	0.000	0.000	0.000	0.000
p-value(Planner's)	0.000	0.000	0.449	0.000	0.000	0.000
p-value (Leg. Barg.)	0.142	0.593	0.002	0.547	0.027	0.001

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Planner's): indicates the p-value of a test where the null hypothesis is that the estimate equals the planner's solution
p-value(Leg. Barg.): indicates the p-value of a test where the null hypothesis is that the estimate equals the legislative bargaining solution

Table 16: Quantile regression output for period 1 investment decisions: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	10.000***	10.000***	10.000***	10.000***	32.500***	10.000
Std.Err.	3.464	1.234	3.363	2.116	9.841	7.531
p-value	0.004	0.000	0.003	0.000	0.001	0.186
T	22.500***	0.000	45.000***	15.000**	22.500*	15.000*
Std.Err	7.630	3.776	6.687	6.932	11.712	8.717
p-value	0.003	1.000	0.000	0.031	0.004	0.000
p-value(DIH)	0.395	0.000	0.000	0.000	0.055	0.087

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

p-value(DIH): Indicates the p-value of the hypothesis test for the Dynamic Inefficiency Hypothesis. For the comparison between SB and DB^{low}, the null hypothesis is that the estimate for T is equal to 16. In the case of SB and DB^{high}, the null hypothesis is that the estimate for T is equal to 72. For the comparison between DB^{low} and DB^{high}, the null hypothesis is that the estimate for T is equal to 56.

Table 17: Random effects regression output for period 1 investment decisions: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	16.713***	11.364***	16.713***	11.340***	38.712***	18.946***
Std. Err	2.567	2.027	2.531	1.933	6.169	3.506
p-value	0.000	0.000	0.000	0.000	0.000	0.000
T	21.999***	7.610*	38.487***	19.232***	16.488**	11.652**
Std. Err	6.764	4.012	5.040	3.889	7.553	4.867
p-value	0.001	0.058	0.000	0.000	0.000	0.000
p-value(DIH)	0.375	0.037	0.000	0.000	0.029	0.017

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

p-value(DIH): Indicates the p-value of the hypothesis test for the Dynamic Inefficiency Hypothesis. For the comparison between SB and DB^{low}, the null hypothesis is that the estimate for T is equal to 16. In the case of SB and DB^{high}, the null hypothesis is that the estimate for T is equal to 72. For the comparison between DB^{low} and DB^{high}, the null hypothesis is that the estimate for T is equal to 56.

Table 18: Quantile regression output for period 1 investment decisions: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, all 10 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	15.000***	10.000***	15.000***	10.000***	32.500***	10.000
Std.Err.	3.088	1.635	3.757	1.542	9.297	7.977
p-value	0.000	0.000	0.000	0.000	0.000	0.211
T	17.500*	0.000	40.000***	20.000***	22.500**	20.000**
Std.Err.	9.557	9.776	3.992	4.803	9.642	9.660
p-value	0.067	1.000	0.000	0.000	0.020	0.039
p-value(DIH)	0.876	0.103	0.000	0.000	0.000	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB),DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

p-value(DIH): Indicates the p-value of the hypothesis test for the Dynamic Inefficiency Hypothesis. For the comparison between SB and DB^{low}, the null hypothesis is that the estimate for T is equal to 16. In the case of SB and DB^{high}, the null hypothesis is that the estimate for T is equal to 72. For the comparison between DB^{low} and DB^{high}, the null hypothesis is that the estimate for T is equal to 56.

Table 19: Random effects regression output for period 1 investment decisions: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, all 10 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	19.883***	14.085***	19.883***	13.949***	41.846***	19.798***
Std.Err.	3.010	2.265	2.967	2.270	7.022	2.798
p-value	0.000	0.000	0.000	0.000	0.000	0.000
T	21.963***	5.796*	39.179***	20.333***	17.216**	14.474***
Std.Err.	7.732	3.295	3.966	3.172	7.499	3.567
p-value	0.005	0.079	0.000	0.000	0.022	0.000
p-value(DIH)	0.440	0.002	0.000	0.000	0.000	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

p-value(DIH): Indicates the p-value of the hypothesis test for the Dynamic Inefficiency Hypothesis. For the comparison between SB and DB^{low}, the null hypothesis is that the estimate for T is equal to 16. In the case of SB and DB^{high}, the null hypothesis is that the estimate for T is equal to 72. For the comparison between DB^{low} and DB^{high}, the null hypothesis is that the estimate for T is equal to 56.

Table 20: Quantile regression output for period 1 investment inefficiencies: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	24.500*	2.000	4.500	-10.500***	-55.500*	-48.000***
Std.Err.	13.249	2.681	8.822	3.782	30.445	13.596
p-value	0.065	0.457	0.610	0.006	0.069	0.001
T	-6.500	16.000***	13.500***	28.500***	33.500***	41.000***
Std.Err.	8.681	2.056	3.856	2.915	11.034	5.308
p-value	0.454	0.000	0.001	0.000	0.003	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 21: Random effects regression output for period 1 investment inefficiencies: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	17.285**	8.246	-5.470	-9.724***	-73.736***	-63.641***
Std.Err.	8.094	5.332	4.377	3.355	20.458	12.497
p-value	0.033	0.122	0.211	0.004	0.000	0.000
T	-5.999	8.390**	16.757***	26.384***	39.512***	44.348***
Std.Err.	6.764	4.012	2.520	1.945	7.553	4.867
p-value	0.375	0.037	0.000	0.000	0.000	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 22: Quantile regression output for welfare (all proposals and proposals that involve MWCs)

	SB		DB^{low}		DB^{high}	
	All	MWC	All	MWC	All	MWC
Estimate	94.164***	94.164***	107.968***	103.500***	180.000***	169.594***
Std. Err.	3.194	3.204	4.104	11.219	6.594	8.918
p-value	0.000	0.000	0.000	0.000	0.000	0.000
p-value(Planner's)	0.000	0.000	0.000	0.043	0.001	0.000
p-value (Leg. Barg.)	0.069	0.070	0.638	0.570	0.928	0.274

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: W, Control: Constant
p-value(Planner's): indicates the p-value of a test where the null hypothesis is that the estimate equals the planner's solution
p-value(Leg. Barg.): indicates the p-value of a test where the null hypothesis is that the estimate equals the legislative bargaining solution

Table 23: Random effects regression output for welfare (all proposals and proposals that involve MWCs)

	SB		DB^{low}		DB^{high}	
	All	MWC	All	MWC	All	MWC
Estimate	91.507***	91.575***	97.557***	84.679***	169.381***	166.999***
Std. Err.	0.685	0.977	4.890	2.603	5.591	4.663
p-value	0.000	0.000	0.000	0.000	0.000	0.000
p-value(Planner's)	0.000	0.000	0.000	0.000	0.000	0.000
p-value (Leg. Barg.)	0.000	0.000	0.012	0.000	0.073	0.008

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: W, Control: Constant
p-value(Planner's): indicates the p-value of a test where the null hypothesis is that the estimate equals the planner's solution
p-value(Leg. Barg.): indicates the p-value of a test where the null hypothesis is that the estimate equals the legislative bargaining solution

Table 24: Quantile regression output for welfare inefficiencies: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	18.336***	18.336***	18.336***	18.336***	18.632***	23.100***
Std.Err.	2.028	1.501	2.504	1.696	3.859	8.618
p-value	0.000	0.000	0.000	0.000	0.000	0.008
T	0.296	4.764	3.564	13.970	3.268	9.206
Std.Err.	4.402	9.691	6.259	11.536	7.377	12.604
p-value	0.946	0.624	0.569	0.227	0.658	0.466

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: ΔW^T, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 25: Random effects regression output for welfare inefficiencies: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	20.993***	21.195***	20.993***	20.461***	29.043***	40.211***
Std.Err.	0.613	1.078	0.605	0.676	4.317	1.918
p-value	0.000	0.000	0.000	0.000	0.000	0.000
T	8.050*	20.595***	11.527**	14.282***	3.476	-6.121
Std.Err.	4.421	2.529	5.270	4.479	6.786	5.077
p-value	0.069	0.000	0.029	0.001	0.608	0.228

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Dependent variable: ΔW^T, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 26: Quantile regression output for welfare inefficiencies: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, all 10 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	8.241	17.027**	2.814	6.610	-13.467	-24.641
Std.Err.	6.735	8.653	6.678	4.414	24.948	25.913
p-value	0.221	0.050	0.674	0.135	0.589	0.343
T	5.019	1.309	10.446*	11.726***	15.873	22.143**
Std.Err.	4.501	7.598	5.917	3.001	11.878	9.776
p-value	0.265	0.863	0.078	0.000	0.182	0.024

Notes: Excludes 32 out of 420 observations in treatment DB^{low} where $W = 0$. Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Dependent variable: ΔW^T, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 27: Random effects regression output for welfare inefficiencies (all proposals and proposals that involve MWCs, all 10 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	11.413***	13.081*	7.378***	10.007***	-4.699	6.328
Std.Err.	4.405	7.421	2.716	2.645	16.226	21.291
p-value	0.010	0.078	0.007	0.000	0.772	0.766
T	6.191	6.612	10.226***	9.261***	14.252**	10.696
Std.Err.	4.283	6.979	2.587	1.806	6.628	7.494
p-value	0.148	0.343	0.000	0.000	0.032	0.154

Notes: Excludes 32 out of 420 observations in treatment DB^{low} where $W = 0$. Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Dependent variable: W, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

Table 28: Random effects regression output for welfare: Pairwise comparisons across treatments (all proposals and proposals that involve MWCs, last 5 matches)

	SB and DB^{low}		SB and DB^{high}		DB^{low} and DB^{high}	
	All	MWC	All	MWC	All	MWC
Constant	91.507***	91.305***	91.507***	92.039***	97.557***	86.389***
Std.Err.	0.613	1.078	0.605	0.676	4.317	1.918
p-value	0.000	0.000	0.000	0.000	0.000	0.000
T	6.050	-6.495**	77.873***	75.118***	71.824***	81.421***
Std.Err.	4.421	2.529	5.270	4.479	6.786	5.077
p-value	0.171	0.010	0.000	0.000	0.000	0.000
p-value(DIH)	0.069	0.000	0.029	0.001	0.608	0.228

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%; Dependent variable: W, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is from DB^{low} (SB), DB^{high} (SB), and DB^{high} (DB^{low}) for each corresponding case.

p-value(DIH): Indicates the p-value of the hypothesis test for the Dynamic Inefficiency Hypothesis. For the comparison between SB and DB^{low}, the null hypothesis is that the estimate for T is equal to 14.1. In the case of SB and DB^{high}, the null hypothesis is that the estimate for T is equal to 89.4. For the comparison between DB^{low} and DB^{high}, the null hypothesis is that the estimate for T is equal to 75.3.

Appendix D: Further Analysis at the Individual Level

Distribution of Investment in Period 1

Figure 6 displays the distribution of period 1 investment by treatment and by strategy type for the last 5 matches of each session.

Distribution of Private Allocations

Private allocations capture how the budget remaining after public investment is divided among committee members and Table 29 summarizes the relevant information. For each treatment there are two statistics: the allocation to the proposer (x^{Pr}) and the allocation to a non-proposer in the coalition (x^{NPr}).[45] The 'theory' values presented in the table correspond to the bargaining equilibrium for Type MM proposals and to the identified theoretical candidate within each type in other cases.

Proposals involving MWCs should reflect proposer power: allocations should theoretically double that of the Non-Proposer coalition member. To test for the presence of proposal power we construct the ratio between the allocation to the proposer and the non proposer in the coalition (x^{Pr}/x^{NPr}). We then use a random effects model and regress that ratio on a constant (see footnote 18 for more details). Under the null hypothesis of no proposal power, the coefficient estimated on the constant is not different than one. We can reject the null only in the case of Type MM proposals in the SB treatment. In that case, there is a small presence of proposal power at levels comparable to previous reports in the literature (see for example Fréchette et al. (2003)). There is no evidence of proposal

[45] In proposals satisfying definition A, x^{NPr} is computed for each proposal as the average to both coalition members other than the proposer.

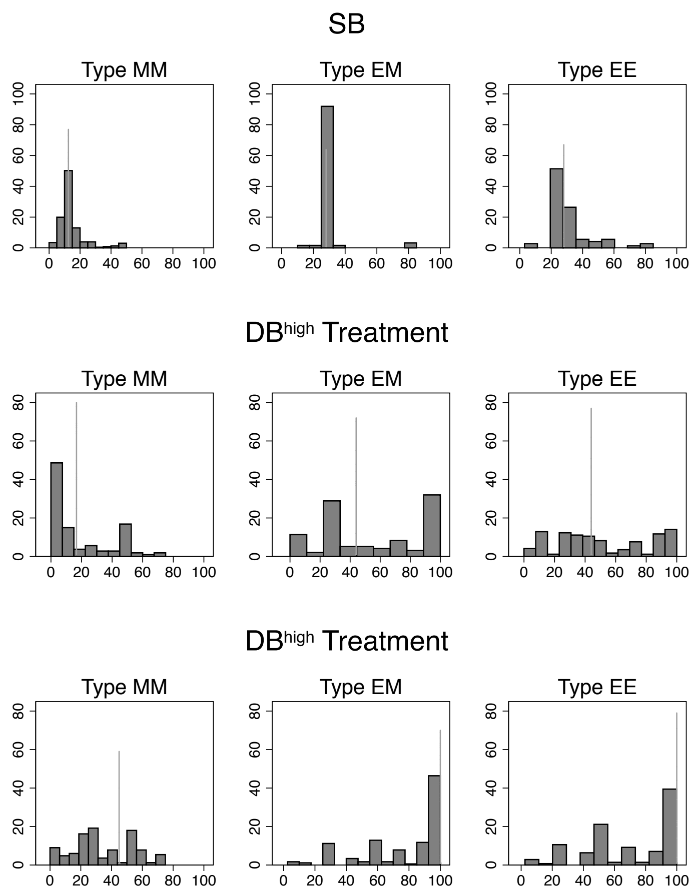

Figure 6: Distribution of Period 1 Investment as % of Budget
Lines: Theory predictions

Table 29: Private allocations as % of Budget

Treatment		Type MM				Type EM				Type EE			
		Period 1		Period 2		Period 1		Period 2		Period 1		Period 2	
		Theory	Med.	Theory	Med	Theory	Med	Theory	Med	Theory	Med	Theory	Med
SB	x^{Pr}	58.4	45.0	58.4	45.0	24.0	25.0	58.4	45.0	24.0	25.0	24.0	25.0
	x^{NPr}	29.1	42.5	29.1	42.5	24.0	25.0	29.1	45.0	24.0	25.0	24.0	25.0
DB^{low}	x^{Pr}	55.6	45.0	60.1	50.0	18.7	16.5	60.1	50.0	18.7	20.0	26.9	25.0
	x^{NPr}	27.8	45.0	30.3	49.0	18.7	15.0	30.3	50.0	18.7	20.0	26.9	25.0
DB^{high}	x^{Pr}	36.8	35.0	66.7	49.5	0	5.0	66.7	50.0	0	10.0	33.3	23.5
	x^{NPr}	18.4	35.0	33.3	45.0	0	5.0	33.3	50.0	0	10.0	33.3	23.5

Theory: Predicted values; Med: Observed median

x^{Pr}: Allocation to proposer

x^{NPr}: Allocation to non-proposer who is a coalition member

Table 30: Transitions from type in match 6 to latter matches

Treatment	Type in Match 6	Prob. selects each strategy type in matches 7-10 (in %)			
		Type MM	Type EM	Type EE	Other
SB	Type MM	82.5	13.3	0.0	4.2
	Type EM	20.8	42.5	8.3	12.5
	Type EE	8.3	29.2	25.0	37.5
	Other	41.7	0.0	0.0	58.3
DB^{low}	Type MM	71.2	9.6	9.6	9.6
	Type EM	36.1	61.1	0.0	2.8
	Type EE	2.9	27.9	64.7	4.4
	Other	16.7	58.3	25.0	0.0
DB^{high}	Type MM	77.6	10.5	6.6	5.3
	Type EM	13.8	78.8	7.5	0.0
	Type EE	4.2	37.5	52.1	6.3
	Other	50.0	8.3	0.0	41.7

power in DB treatments.

Strategy Types as the session evolves

For each subject we fix the strategy type they select in match 6 and we compute the likelihood they select each possible type in matches 7-10. We report the probabilities in Table 30.

A few patterns emerge. First, there is a high persistence of the most popular strategy types. Type MM strategies are the most popular in the SB treatment and a large majority of subjects selecting those strategies in match 6 (82.5%), stick to that type for the remainder of the session. Similar conclusions apply, for example, for Type MM and Type EM strategies in the DB^{high} treatment. Second, for those who select Type EM in match 6, but change afterwards the most popular transition is towards Type MM strategies. Third, for those who select Type EE in match 6, the most popular switch is to Type EM proposals.

Table 31: Proportion of subjects who use one, two, or all strategy types

	Treatment		
	SB	DB^{low}	DB^{high}
Only Type MM	15.6%	4.8%	9.3%
Only Type EM	0.0%	0.0%	0.0%
Only Type EE	0.0%	9.5%	3.7%
Only Type MM or Type EM	31.1%	26.2%	29.6%
Only Type MM or Type EE	15.6%	9.5%	3.7%
Only Type EM or Type EE	15.5%	28.6%	29.6%
All Types$^{\times}$	22.2%	21.4%	24.1%

Notes: $^{\times}$ All types involve subjects who used Type MM, Type EE and Type EM at some point in the experiment. It is possible that some of these subjects also used strategies classified as "Other."

An alternative point of view to study how subjects transition among strategies is to consider the proportion of subjects who use only one strategy type or more during a session. Table 31 shows for each treatment the proportion of subjects who use only one type, two types or all types. Slightly more than 20% of subjects across treatments use all three strategy types. Most subjects use two strategy types with the groups: 'Type MM and Type EM' and 'Type EM and Type EE' being the most popular.

Strategies of type MM and EM: Payoff Comparison

Given that strategies type MM and type EM remain popular in both DB treatments towards the end of the session, we inspect further differences in payoffs between them. To do this we take the place of a period 1 proposer and consider her alternatives. That is, we focus on subjects who were proposers in period 1 and compare earnings depending on the value of A1 (see section 5.4 for a definition).

For type EM proposals to be attractive, there should be a period 2 reward. Thus, we look at period 2 payoffs of period 1 proposers, $u_2 = x_2^{P1} + f(g_2)$, and estimate $u_2 = \alpha_0 + \alpha_1 A1 + \epsilon$. A positive estimate for α_1 would suggest that using a strategy type EM in period 1 increases payoffs in period 2. The last column of Table 32 presents the estimates for α_1, which are significant at the 5% level for all treatments. Quantitatively, additional payoffs imply an extra 15% in the second period. The extra gain in period 2 comes at the cost of a lower period 1 payoff: in comparison to the bargaining solution, a proposal closer to the planner's implies higher payoffs for the committee as a whole, but lower payoffs for the proposer. Table 32 shows mean period 1 payoffs to the proposer, depending on proposal type. There are negligible differences between type EM and EE proposals, but such proposals lag compared to type MM: on average they represent between 72 and 76% of type MM payoffs depending on treatment. Yet, if we add the additional period 2 payoff the differences shrink: a proposal of type EM involves between 86 and 89% of the payoffs to a type MM proposal.

Table 32: Period 1 Payoff Levels: Accepted Proposals

Treatment	Mean Period 1 Payoffs to proposers			Additional Period 2
	Type MM	Type EM	Type EE	Payoff if A1=1
SB	112.6	85.5	85.4	13.6
DB^{low}	107.4	80.1	81.7	15.2
DB^{high}	105.5	76.0	78.2	14.5

Table 33: Type EM strategies: Punishment and Rewards Random Effects Estimates

	SB		DB^{low}		DB^{high}	
Variable	Coeff.	Rob. Sd. Err.	Coeff.	Rob. Sd. Err.	Coeff.	Rob. Sd. Err.
Constant	20.50***	(1.26)	19.52***	(2.35)	22.29**	(1.25)
$A1$	2.60***	(0.98)	1.79	(2.89)	-2.59	(1.31)
$EM_{strategy}$	-6.36	(4.56)	-9.07***	(2.63)	-2.74***	(3.08)
$A1 \times EM_{strategy}$	4.92	(13.29)	15.54**	(7.97)	12.09***	(4.07)
Number of Observations	300		280		360	

Notes: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Type EM strategies: Punishments and Rewards estimates

Table 33 provides the estimates used for our computations in Table 9.

Determinants of Voting

Now we study the features that determine whether a proposal passes or not. This exercise can also help to understand why some strategy types are selected. If, for example, a proposal type EM or type EE was more likely to be voted up in period 1, then it may be more likely to be selected vis a vis a proposal involving a MWC. We focus on stage 1, period 1 proposals that were submitted for voting and estimate

$$vote_{im} = 1\left\{\beta_0 + \beta_1\left(f(I_1)_{im}\right) + \beta_2 x_{1,im} + \beta_3 x_{1,im}^{Pr} + \beta_4 E_{im} + \beta_5 MWC_{im} + \alpha_i + \nu_{im} \geq 0\right\},$$

where $vote_{im}$ is a dummy variable that takes value 1 if subject i approved the proposal in match m, and $1\{\cdot\}$ is an indicator function that takes value one if the left-hand side of the inequality inside the braces is greater than or equal to zero and the value zero otherwise. Explanatory variables include the investment payoff ($f(I_1)$), the private period 1 allocation (x_1), the allocation to the proposer (x_1^{Pr}) and dummy variables for whether the proposal involves an Equal Split (E) or a MWC. The equation is estimated using a random effects probit, with a one-way subject error component for all rounds. Table 34 reports the corresponding marginal effects.

The coefficients provide evidence that there is a positive effect of investment payoff and private allocations on the probability of voting positively for a proposal and a negative effect from higher private allocations to the proposer. This is consistent with previous estimates in the literature (see Fréchette et al. (2003, 2005)). Estimates on the Equal split and MWC dummies are positive, but not significant in the DB treatments. This suggests that there is not a particular gain in terms of

Table 34: Period 1 Voting: Random Effects Probit (Marginal Effects)

	SB		DB^{low}		DB^{high}	
Variable	Coefficient	Rob. Std. Err.	Coefficient	Rob. Std. Err.	Coefficient	Rob. Std. Err.
Investment Payoff	0.023*	(0.013)	0.032***	(0.010)	0.039***	(0.011)
Private Allocation	0.057***	(0.067)	0.050***	(0.007)	0.048***	(0.006)
Proposer Private Allocation	-0.023***	(0.007)	-0.023***	(0.006)	-0.025***	(0.005)
MWC	0.608*	(0.335)	0.230	(0.372)	0.045	(0.254)
Equal Split	0.118	(0.210)	0.254	(0.268)	0.226	(0.228)
Number of Observations	450		420		540	

Notes: *Significant at 10%; **Significant at 5%; ***Significant at 1%;

Only the first stage of proposals for each round is considered.

Investment Payoff=5(investment)$^{0.5}$

higher likelihood of a proposal passing because it involves an Equal Split or a MWC.

Tables with support for statistical tests

Table 35: Quantile regression output for period 1 investment decisions

	Type MM			Type EM			Type EE		
	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}
Estimate	10.000***	50.000***	30.000***	25.000***	50.000***	85.000***	25.000***	50.000***	70.000***
Std. Err.	1.023	11.869	5.226	0.388	11.869	15.850	0.187	11.869	4.767
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value (Theory)	0.015	0.006	0.005	0.614	0.000	0.345	0.000	0.614	0.563

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Theory): indicates the p-value of a test where the null hypothesis is that the estimate equals the theoretical value in Table tab:InvStrategies

Table 36: Random effects regression output for period 1 investment decisions

	Type MM			Type EM			Type EE		
	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}
Estimate	13.623***	19.792***	33.208***	27.848***	51.542***	73.453***	30.202***	50.666***	66.886***
Std. Err.	3.156	1.953	3.174	1.127	7.422	1.996	3.238	8.971	4.216
p-value	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
p-value (Theory)	0.722	0.113	0.000	0.893	0.309	0.000	0.496	0.458	0.000

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Theory): indicates the p-value of a test where the null hypothesis is that the estimate equals the theoretical value in Table tab:InvStrategies

Table 37: Quantile regression output for period 2 investment decisions

	Type MM			Type EM			Type EE		
	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}
Estimate	10.000***	0.000	5.000	10.000***	0.000	0.000	25.000***	25.000***	31.000*
Std. Err.	1.245	1.181	6.289	3.592	0.600	0.000	0.795	3.277	16.957
p-value	0.000	1.000	0.428	0.007	1.000	-	0.000	0.000	0.070
p-value (Theory)	0.046	0.000	0.423	0.489	0.000	-	0.000	0.084	0.000

Notes: Standard Errors clustered by session whenever possible. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Theory): indicates the p-value of a test where the null hypothesis is that the estimate equals the theoretical value in Table tab:InvStrategies

Table 38: Random effects regression output for period 2 investment decisions

	Type MM			Type EM			Type EE		
	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}
Estimate	12.289***	7.542***	12.986***	12.702***	7.914***	10.962***	30.225***	33.869***	41.323***
Std. Err.	2.166	2.899	0.736	4.058	1.470	2.601	2.480	3.156	11.404
p-value	0.000	0.009	0.000	0.002	0.000	0.000	0.000	0.000	0.000
p-value (Theory)	0.923	0.567	0.000	0.960	0.382	0.000	0.369	0.000	0.000

Notes: Standard Errors clustered by session whenever possible. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant
p-value(Theory): indicates the p-value of a test where the null hypothesis is that the estimate equals the theoretical value in Table tab:InvStrategies

Table 39: Random effects regression output for investment comparisons across types

	Type MM v Type EM			Type MM v Type EE			Type EM v Type EE		
	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}	SB	DB^{low}	DB^{high}
Constant	14.921***	16.985***	31.887***	13.833***	20.077***	32.997***	28.321***	50.481***	74.520***
	3.111	6.066	3.033	2.560	1.769	4.198	1.240	6.941	1.905
	0.000	0.005	0.000	0.000	0.000	0.000	0.000	0.000	0.000
T	9.037*	36.081***	40.903***	18.083***	29.946***	36.407***	1.996	2.306	-2.891
	4.661	3.297	4.491	4.066	9.066	4.702	1.753	2.987	2.142
	0.053	0.000	0.000	0.000	0.001	0.000	0.255	0.440	0.177

Notes: Standard Errors clustered by session. *Significant at 10%; **Significant at 5%; ***Significant at 1%;
Dependent variable: Investment, Control: Constant and treatment dummy T, that takes value 1 (0) if the observation is Type EM (Type MM), Type EE (Type MM), and Type EE (Type EM) for each corresponding case.